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AN INVESTIGATION INTO VARIABLES MODERATING THE OUTCOMES OF
MINDFULNESS MEDITATION

Section A: A systematic review of variables moderating the outcomes of Mindfulness-based
Cognitive Therapy (MBCT) for depression

Word Count: 8000 (8400)

Section B: Adapting brief mindfulness meditation to the individual: Consideration of trait
self-criticism

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Summary of MRP Portfolio

Section A:

It is now accepted that Mindfulness-based Cognitive Therapy (MBCT) is an effective treatment for the prevention of relapse in depression, although there is limited research into what factors moderate participant outcomes. Section A adds to the limited literature on what variables moderate the outcomes of MBCT for depression by conducting an up-to-date explorative systematic narrative literature review. Twelve studies were included, and good evidence was found for the moderating effect of the number of previous depressive episodes on rate of relapse to depression, although this was not the case for the outcome of depression severity. Evidence for numerous other potential moderators and their impact on differing outcomes was described.

Section B:

An experimental mixed-measures design compared two types of brief mindfulness meditation (MM) in a non-clinical population ($n = 70$): *acceptance-oriented* and *concentration-oriented* MMs. An interaction between these MMs and a person's level of self-criticism (*high vs low*) was hypothesised to moderate the outcome of state positive affect. The primary hypothesis was not confirmed, although secondary hypotheses were, and high self-criticism was found to be predictive of improvements in negative affect following both MMs. These results are discussed with regards to future research and the application of MBCT.

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Section A: Literature Review

A systematic review of variables moderating the outcomes of Mindfulness-based Cognitive Therapy (MBCT) for depression

Words count: 8000 (8400)

Abstract

There is now good evidence demonstrating that mindfulness-based cognitive therapy (MBCT) is effective in reducing the rate at which people with a history of three or more episodes of major depression relapse. Previous reviews have also begun to document variables which moderate the outcomes of MBCT in an effort to elucidate who may be more or less likely to benefit from the intervention. The current review aimed to build upon these reviews to investigate moderators of MBCT by conducting an up-to-date systematic review of the literature. Method: The databases PsychINFO, MEDLINE, the Cochrane Database of Systematic Reviews, PubMed and Web of Science were searched, resulting in twelve studies being included for review. Bias ratings were generally low, whereas the quality rating of moderator analyses was highly varied.

A wide range of potential moderators was reported, with outcomes primarily being the rate of relapse to depression and depressive symptom severity. There was good evidence that the number of previous depressive episodes moderates rates of relapse to depression following MBCT, but not depressive symptom severity. Other documented moderators included childhood adversity, brooding, cognitive reactivity and antidepressant usage. Clinical implications including MBCT entry criteria are suggested, in addition to areas for future research.

Key words – mindfulness-based cognitive therapy, MBCT, depression, moderation

Introduction

In 2018, the World Health Organisation (WHO) stated that “depression is the leading cause of ill health and disability worldwide” (World Health Organisation, 2018). Whilst there exists a wide range of treatments which have proven effective in alleviating depression, the risk of episodes of depression reoccurring following recovery from an initial episode has been demonstrated to be at least 50%, increasing to up to 90% after two episodes of depression (Burcusa & Iacono, 2007; Kupfer, Frank, & Wamhoff, 1996; Post, 1992; Williams, Crane, Barnhofer, Van Der Does, & Segal, 2006). In addition to furthering depression’s substantial societal economic burden (Sobocki, Jönsson, Angst, & Rehnberg, 2006), recurring episodes continue to impact negatively on factors such as people’s quality of life, ability to work, well-being and cardiovascular health (Lépine & Briley, 2011; Mascha, Koeter, Bockting, & Schene, 2010), in addition to increasing the risk of self-harm and suicide (Williams, Duggan, Crane, & Fennell, 2006; Williams et al., 2006). Clearly then, there exists a pressing need for approaches able to reduce the risk of depressive episode recurrence.

Mindfulness-based Cognitive Therapy (MBCT) represents one such approach which has now demonstrated reliable effectiveness in reducing the rates of relapse to depression for those currently in remission and who have experienced three or more previous episodes (Piet & Hougaard, 2011), and it is currently a recommended relapse-prevention treatment for this population in the National Institute of Health and Care Excellence (NICE) guidelines (NICE, 2009). MBCT was developed by Segal, Williams and Teasdale (2002) who drew upon a model of cognitive vulnerability for depressive relapse. This model posits, and subsequent research supports (e.g. Ingram, Miranda, & Segal, 1998; Segal, Gemar, & Williams, 1999), that the thinking styles activated by mildly depressive mood are likely to differ between those who have previously experienced an episode of depression and those who have not. The powerfully negative, self-devaluative, and hopeless thinking styles and thought content which characterise

a primary major depressive episode are argued to be re-activated when a person who is in remission experiences mildly depressed mood; whereas those never having experienced a depressive episode would not have internalised these thinking styles which would therefore not be evoked when experiencing mildly depressed mood (Ma & Teasdale, 2004; Teasdale, 1988; Teasdale, 1997). Teasdale (1997) argued that increasing levels of rumination upon the mildly depressed mood and associated thoughts in those individuals with a history of major depression – where such rumination may be deployed as a maladaptive coping mechanism (Spasojevic & Alloy, 2001) – may ultimately lead to an increased probability of depressive relapse. This assertion is well-supported by the literature demonstrating the significant causal and maintenance role of rumination in depression (Crane & Williams, 2010; Segal, Williams, & Teasdale, 2012; Spasojevic & Alloy, 2001; Treynor, Gonzalez, & Nolen-Hoeksema, 2003). Furthermore, this association between mildly depressed mood and depressive thinking style in people who previously experienced depression is thought to become more strongly conditioned as more episodes of depression entrench it; this is then seen to lead to easier activation of such a thinking style by stressful life events, explaining why the risk of depression recurrence increases with each episode (Kendler, Thornton, & Gardner, 2000; Lewinsohn, Allen, Seeley, & Gotlib, 1999; Post, 1992).

MBCT involves the application of a combination of mindfulness and cognitive therapy techniques for depression which are grounded in the above theory (Segal et al., 2012). It focuses predominantly on increasing the non-judgemental and accepting awareness of which thinking style (or ‘mode’ of mind) and its corresponding affects and cognitions is currently activated, in addition to cultivating focused attention. This is seen to allow for disengagement from negative, depressive and ruminative ‘modes’ of mind towards engagement with present-moment experience (termed the ‘being’ mode). ‘Engagement in the ‘being’ mode is seen to prevent the process of rumination resulting from engagement in the ‘driven-doing’ mode.

Whilst MBCT's effectiveness in reducing relapse to depression is accepted, as with all interventions, it does not demonstrate improvements for all people. For example, in the two seminal research papers comparing MBCT to treatment as usual (TAU), over one third of participants in the trials experienced a recurrence of depression in the 12 months following MBCT (Ma & Teasdale, 2004; Teasdale et al., 2000). Therefore, it is important to consider who may or may not benefit from taking part in order to most effectively channel funding, avoid the potentially demoralising experience for those who do not find MBCT of help, and to generate research into ways that MBCT might be modified in order to accommodate for those identified by particular characteristics as unlikely to benefit. In other words, it is important to consider what works and for whom.

One approach to this problem is to identify variables which moderate MBCT outcomes. A moderator has been defined by Baron and Kenny (1986) as "a variable that affects the direction and/or strength of the relation between an independent or predictor variable and a dependent or criterion variable". Two previous systematic reviews of the MBCT and depression literature that include moderators of outcomes exist. In their meta-analysis of trials aiming to treat depressive symptoms (as opposed to reducing relapse rates) with MBCT, Lenz, Hall and Smith (2016) conducted their own moderation analyses examining the impact of both sample characteristics (*participant age, country of study, percentage of men and women*), and study characteristics (*study setting, type of control condition/active treatment comparison*) on depressive symptoms across 31 studies, although they do not state a theoretical reason for choosing these characteristic variables. Moderation effects were documented for: *participant age, percentage of men, domicile, study setting* and *type of alternative condition comparison*.

Kuyken et al. (2016) analysed individual patient data outcomes from nine randomised controlled trials (RCTs) of MBCT for those in full or partial remission from depression,

showing that *age, sex, education, relationship status, age of first depression and number of previous episodes* did not statistically moderate relapse rates following MBCT.

The current review aimed to update these two publications, which included papers published up to November 2014 only. This review was exploratory in nature as it was felt that overly restricting the type of moderator included may miss other potential variables of interest. However, there was still a focus on person-level variables as opposed to aspects of the intervention or control(s) as these would not aid identification of people more or less likely to benefit from MBCT. Further, it was decided that including studies documenting a range of outcome variables would be important for informing the wider question of what works and for whom with regard to MBCT for depression. This contrasts with Kuyken et al. (2016) including only rate of relapse of depression as an outcome, and Lenz et al.'s (2016) review including only depressive symptoms.

Method

Literature Search

Broad search terms were used to scope the literature base, which provided the final set of search terms used below. A final search was conducted on 3rd August 2018 using five electronic databases: PsychINFO, MEDLINE, the Cochrane Database of Systematic Reviews, PubMed and Web of Science. The search terms were: (MBSR¹ OR Mindfulness-based stress reduction OR MBCT OR Mindfulness-based cognitive therapy) AND ((randomised controlled trial OR randomized controlled trial OR RCT) OR (meta-analys* OR meta-regression OR systematic review)). The inclusion of systematic reviews or meta-analyses/regressions was to check for any additional papers which may not have been detected using the other search terms. Finally, no date restrictions were applied to the search.

¹Originally, a wider-focused review was anticipated, but upon discovering the volume of the literature, a decision was made to narrow the focus of the review to only MBCT.

Inclusion and Exclusion Criteria

Studies were included if: they included an adult sample; participants were currently depressed or had previously experienced depression; they employed a randomised-controlled trial (RCT) experimental design, or reported on the data from another RCT study; they examined whether at least one variable moderated the outcome(s) of a group-based Mindfulness-Based Cognitive Therapy (MBCT) intervention; and the moderator(s) examined were not exclusively characteristics of the intervention studied or the control group (for example, the length of mindfulness practice). Group-based MBCT interventions were defined as those reported to have followed the original 8-week MBCT protocol created by Segal, Williams and Teasdale (2002), allowing for minor modifications. Studies were excluded if: they were not written in English, due to lacking enough resource to access reliable translation services; they employed qualitative methodologies which preclude comparison of results across studies; or they reported neurological or physiological outcomes only.

Results

The full texts of the first 200 papers meeting all inclusion criteria in their title or abstract, except for reporting the measurement of moderation effect, were checked to ensure that there were not moderation analyses contained within the papers which were not reported in the abstract or title. No moderation analyses were found in this search which gave confidence that initial screening of the remaining titles and abstracts was sufficient for identifying relevant studies. This screening resulted in a total of 20 studies, and full text screening reduced this to 12 studies. Reference list screening identified no additional studies, and searches of Google Scholar and the journal *Mindfulness* produced no additional results. The final sample was therefore comprised of 12 studies. Figure 1 displays this selection process within a PRISMA diagram.

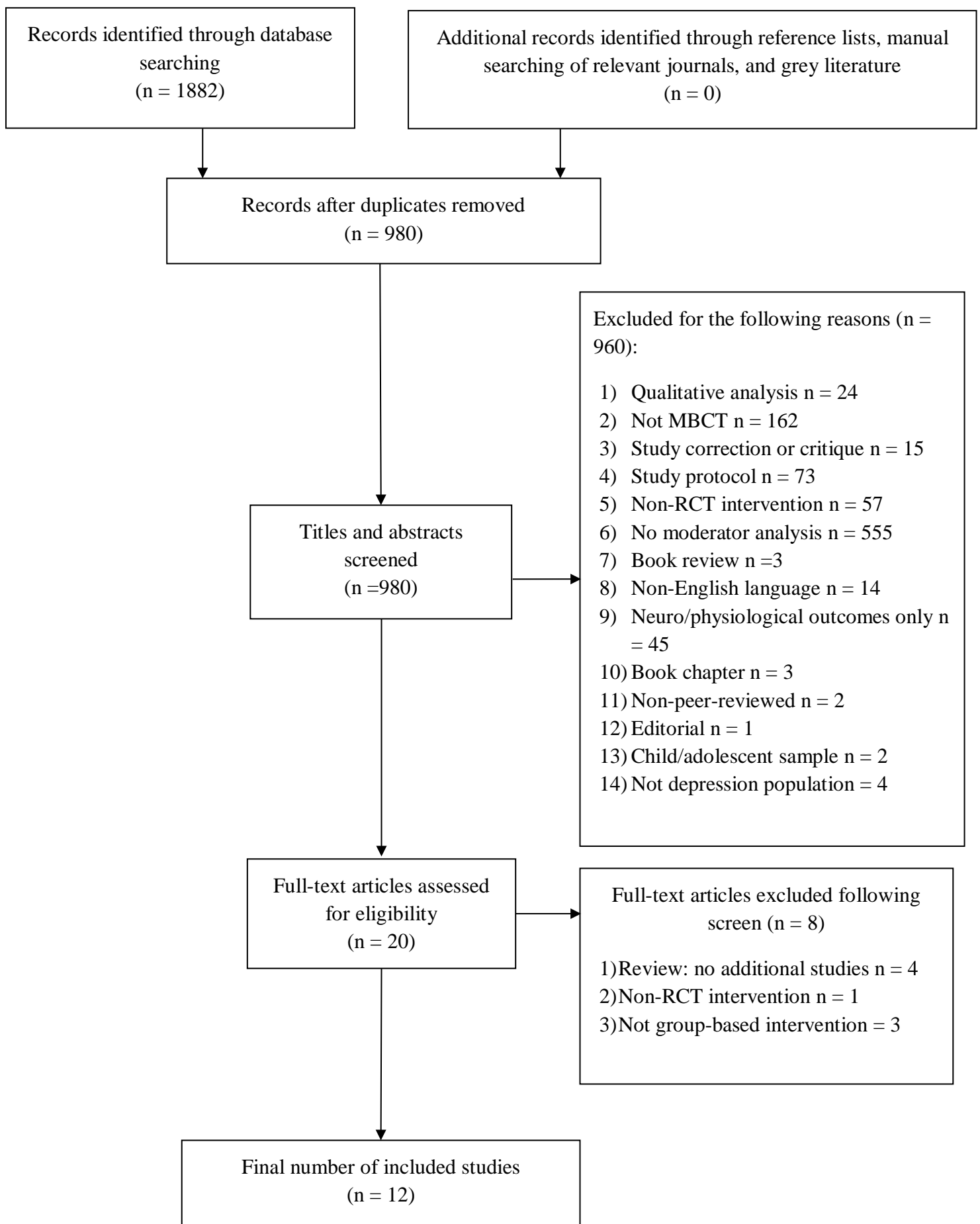


Figure 1: PRISMA flow diagram

Quality Assessment

Assessing both risk of bias (used as an indicator of study quality) of each study, and the quality of individual moderator analyses, is in keeping with similar reviews of moderators of CBT (Knopp, Knowles, Bee, Lovell, & Bower, 2013; Porter & Chambless, 2015). To assess for risk of bias, the Cochrane Risk of Bias II tool was drawn upon (Higgins & Altman, 2008). This was modified to include whether authors had assessed for the effects of participant adherence to the intervention, as research has demonstrated that the amount of formal home practice conducted during MBCT is significantly related to outcomes in depression (Crane et al., 2014). A study's bias rating on the criterion of *allocation concealment* was used to categorise the study's overall risk of bias rating (*low, high, uncertain*), based upon the recommendation of Knopp et al. (2013) that this criterion is a reliable indicator of overall study bias.

The quality of moderation analyses followed a similar format to Knopp et al. (2013), with a number of quality criteria each rated as either: *being met; unclear; not met*. A total quality indication score ranging from 0-7 was then calculated by summing the number of quality criteria being met for each moderation analysis (see Table 3), and this was used as rough indicator of analysis comparison.

Literature review

This review is organised into themes and subthemes which were felt to broadly represent the range of variables reported in the literature to have been analysed for their possible moderation effects on the outcomes of MBCT. See Table 1 for an overview of the included studies' characteristics, Table 2 for risk of bias ratings, and Table 3 for moderation analyses quality rating.

Within each theme section, a brief overview of the included studies and their findings is given, in addition to details of their quality and the quality of their moderation analyses, and a summary where applicable. It should be noted that when the term ‘moderation’ is used, this is to signify statistical moderation only, due to these effects being correlational in nature, and not experimentally manipulated.

Mood Disorder

Number of previous depressive episodes. Teasdale and colleagues (2000), in their seminal RCT teaching MBCT to patients in recovery from major depressive disorder (MDD), demonstrated that for those who had experienced three or more previous depressive episodes (from here-on shortened to 3+), MBCT was significantly more effective than treatment as usual (TAU) in preventing relapse to depression over a follow-up period of 12 months. This was contrasted with an absence of such a protective effect in those patients receiving MBCT who had experienced only two previous depressive episodes. The number of previous episodes of depression has therefore had a long-standing presence as a potential moderator of MBCT treatment outcome on relapse rate, and NICE guidelines (NICE, 2009) currently recommend MBCT for preventing relapse only for those with a history of 3+ depressive episodes.

To date, seven studies have tested for the potential moderating effect of this variable on the outcomes of MBCT; four of which examined relapse rate to depression as the primary outcome (Ma & Teasdale, 2004; Segal et al., 2010; Teasdale et al., 2000; Williams et al., 2014); two examined relapse rate and depressive symptom severity (Eisendrath et al., 2016; Kuyken et al., 2010), and one examined depressive symptom severity only (Geschwind, Peeters, Huibers, van Os, & Wichers, 2012). Segal et al. (2010) and Eisendrath et al. (2016) recruited participants who were currently depressed, whereas participants in the other studies were in remission or recovery.

Applying the quality criteria to these studies found the majority to present a low risk of bias, with both Kuyken et al. (2010) and Eisendrath et al. (2016) rated as uncertain risk of bias as they failed to clearly state methods for allocation concealment from experimenters. Moderation analyses were mostly rated as 4+ out of 7, with Eisendrath et al. (2016) lowest at 2. The implications of methodological issues for the findings in relation to each outcome are now considered.

Relapse rate as a primary outcome. When comparing two versus 3+ previous episodes, Teasdale et al. (2000) Ma and Teasdale (2004), and Segal et al. (2010) found the number of previous depressive episodes moderated the efficacy of MBCT with respect to relapse prevention. It is important to note that in Segal et al. (2010) this effect was also found for the maintenance antidepressant medication (M-ADM) control group, which might suggest that this moderation effect applies at least to M-ADM too, and possibly other active treatments. In contrast, Eisendrath et al. (2016) failed to find such an effect, as did those studies including participants with 3+ episodes only (Kuyken et al., 2010; Williams et al., 2014).

Whilst these findings present a mixed picture, the lack of effect reported by Eisendrath et al. (2016) should be interpreted with caution as in addition to the uncertain risk of bias in the study overall, the authors failed to report on their moderation analysis for this outcome variable and merely state in their discussion that no effect was found. Further, they conducted over ten moderation analyses, therefore lowering the precision of the moderation model.

Therefore, if less weight is given to Eisendrath et al.'s (2016) null moderation effect due to this study's questionable quality, the findings might be interpreted as providing evidence in support of the suggestion of Teasdale et al. (2000). These authors hypothesised that participants with 3+ episodes represent a clinical population distinct from those with two or less episodes, with different characteristics and significantly differing outcomes following

MBCT (in favour of the 3+ population), and therefore a moderation effect is only detected in studies including both populations.

Depressive symptom severity as a primary outcome. Studies investigating whether the number of previous episodes was predictive of the outcome of severity of depressive symptoms (as measured by the Hamilton Depression Rating Scale [HAM-D]) post-MBCT, found no evidence of such a relationship (Eisendrath et al., 2016; Geschwind et al., 2012; Kuyken et al., 2010). As two of these studies (Eisendrath et al., 2016; Geschwind et al., 2012) included both above hypothesised participant populations (2 vs 3+ previous episodes), this null result may therefore suggest that whilst it is possible that relapse rate may not be effectively reduced in those participants with two previous depressive episodes, MBCT could be effective in reducing harmful depressive symptomology. However, findings should be treated as preliminary due to the variable quality of the studies in terms of risk of bias and moderation analyses.

Summary. In summary, these findings provide some evidence that those having experienced 3+ episodes of depression demonstrate significantly lower relapse rates following MBCT, compared to those experiencing fewer than three episodes. However, this requires further replication with high quality research to confirm, especially considering the possibility of bias in Kuyken et al. (2010) and Eisendrath et al.'s (2016) results, and the relatively poor quality of Eisendrath et al.'s moderation analysis. Additionally, in the one study involving both 2 and 3+ episode participants and an active control condition (M-ADM), the number of previous episodes of depression was predictive of relapse rate across conditions. This suggests that this statistical moderation effect may not be unique to MBCT, although the moderation quality rating of 5/7 means this should be interpreted with caution. Finally, there is no current evidence to suggest that the number of previous episodes of depression predicts the outcome of depressive symptom severity as measured by the HAM-D. If confirmed with higher quality moderation analyses, this latter finding is important when considering who is offered MBCT.

Previous episode characteristics. In addition to the number of previous depressive episodes, there has also been interest in the literature regarding the possible moderating effect of differing aspects of the quality of these episodes, such as: the recency of a person's last depressive episode (Teasdale et al., 2000); previous episode severity (Ma & Teasdale, 2004); or the age at which a person first experienced a depressive episode (Eisendrath et al., 2016; Kuyken et al., 2010; Williams et al., 2014). However, none of these studies found an effect.

Baseline depression severity. Baseline depressive symptom severity on the HAM-D was entered into moderation analyses across five studies, as both a continuous and categorical variable by Kuyken et al. (2010) and van Aalderen et al. (2012), with Kuyken et al.'s (2010) categorical distinction being between full and partial depressive episode remission, and van Aalderen et al.'s (2012) between current depression and remission. Van Aalderen et al. (2012) also entered Beck Depression Inventory (BDI) scores as a continuous variable. Segal et al. (2010) categorised symptoms as representing stable versus unstable remission, achieved immediately following tapering off antidepressant medication. Williams et al. (2014) entered baseline HAM-D scores as continuous only, in addition to Beck Depression Inventory II (BDI-II) scores, and Eisendrath et al. (2016) entered HAM-D scores as a continuous variable only.

Quality of moderation analyses was mixed, ranging from comparatively low in Eisendrath et al. (2016) - who again failed to report details of their analysis - to high (7) in Segal et al. (2010), and all studies but Segal et al. (2010) tested for over five possible moderators. Risk of bias ratings were also mixed (see above for criticisms of Kuyken et al. (2010) and Eisendrath et al. (2016)), and it was unclear in all studies as to whether authors had followed a pre-specified analysis plan.

Segal et al. (2010) reported that for those participants who had been categorised as achieving an unstable remission - defined as experiencing "symptom flurries" between the

initial antidepressant medication phase of the trial and randomisation – both MBCT and M-ADM significantly reduced the rate of relapse to depression over 18 months, compared to stable remitters. Further, Williams et al. (2014) demonstrated a non-specific effect across MBCT, TAU and their active control of Cognitive Psychological Education (CPE), where elevated HAM-D scores at baseline predicted a greater risk of relapse to depression following the treatment period. However, no such effect was found on the Beck Depression Inventory (BDI-II).

In comparison, the remaining three studies failed to find evidence for baseline HAM-D scores as a predictor of either relapse to depression (Eisendrath et al., 2016; Kuyken et al., 2010) or post-treatment depressive symptom severity (Eisendrath et al., 2016; Kuyken et al., 2010; van Aalderen et al., 2012). Van Aalderen et al. (2012) presented a similar lack of moderation effect of BDI scores, to Williams et al.'s (2014) aforementioned null finding.

Summary. From the above, it is not possible to derive a clear picture of whether baseline depressive symptom severity, including whether participants are currently depressed or in remission, moderates MBCT outcomes. There is some high quality evidence suggesting that the instability of symptom severity may predict an improved response to both MBCT and ADM in terms of post therapy relapse rate of depression (Segal et al., 2010), and this requires replication. In terms of depressive symptom severity itself, one high quality study reported a predictive effect on rate of relapse of depression which was not related to treatment type (Williams et al., 2014), and this is contrasted with the same study's null effect of baseline participant-rated depression scores (on the BDI-II), and the null findings of three other studies. The increased risk of bias in both the Eisendrath et al. (2016) and Kuyken et al. (2010) papers may be one explanation for these conflicting results, in addition to the very poor (0/7) to moderate (4/7) quality of their moderation analyses, respectively, relative to the other included papers.

Childhood Adversity

The distinct 3+ episode participant population proposed by Teasdale et al. (2000) and Ma and Teasdale (2004) was also characterised by the latter authors as having experienced an increased prevalence of childhood adversity, and three subsequent studies were found to have investigated whether levels of childhood adversity are predictive of MBCT outcomes: Eisendrath et al. (2016), Williams et al. (2014), and Michalak, Probst, Heidenreich, Bissantz and Schramm (2016). Williams et al. (2014), in their 3+ participant sample who were in remission or recovery, demonstrated that higher levels of childhood adversity were predictive of reduced rates of relapse of depression following MBCT, compared to an active control of Cognitive Psychological Education or to TAU. This study was rated as presenting a low risk of bias, and the moderation analysis was of high quality. These factors, in addition to the large sample size ($n = 274$), mean there can be a relatively high degree of confidence in the reported moderation effect.

Michalak et al. (2016) recruited participants with three forms of depression diagnosis (see Table 1) and compared depressive symptom severity (HAM-D and BDI-II) following MBCT, TAU or Cognitive Behavioural Analysis System of Psychotherapy (CBASP), entering baseline Childhood Trauma Questionnaire (CTQ) scores into their moderation analysis. This analysis was rated as medium quality (4); one reason being this study was reporting on the original RCT by Michalak Schultze, Heidenreich and Schramm (2015) which did not detail the use of the CTQ or plan a moderation analysis around its effects on outcomes. The reported finding that at 6-month follow-up only, CTQ scores moderated depressive symptom severity on the HAM-D but not the BDI-II - where greater childhood adversity was predictive of a greater reduction on the HAM-D in both treatment conditions – is therefore interpreted with caution as complementing Williams et al.'s (2014) finding. However, due to the lack of clarity around planned moderation analyses in their original paper, it is possible that the present

finding represents a Type I error following the inclusion of multiple possible moderating variables which were then unreported.

Eisendrath et al. (2016), on the other hand, report no moderation effect of childhood adversity on any outcome post-MBCT. This may reflect the relatively poor quality of the study which may have been underpowered to detect an effect due to the large number of possible moderators entered into their model, and their sample size of 173 being considerably lesser than that of Williams et al. (2014). Alternatively, the lack of effect may be due to the difference in sample population, as Eisendrath et al. (2016) administered MBCT to a currently depressed sample with one or more previous episodes of depression.

Summary. The findings of Williams et al. (2014) provide good quality evidence that childhood adversity statistically moderates the rate of relapse of depression following MBCT in those with 3+ previous episodes. However, it is difficult to directly compare this to the moderation effect described by Michalak et al. (2016), or the null result of Eisendrath et al. (2016), due to the differing depression diagnoses of the studies' participants (Michalak et al. (2016) also did not detail the number of previous episodes), in addition to the latter studies' primary outcome being depressive symptom severity. Further, the medium quality of Eisendrath et al. (2016) and Michalak et al.'s (2016) moderation analyses, and the uncertain risk of bias in the Eisendrath et al. (2016) paper mean that their results must be interpreted with caution. However, these results do suggest that childhood adversity is a variable which warrants further research both in its possible moderating effects on rate of relapse to depression, and depressive symptom severity following MBCT, and clarity is needed on which participant population this effect is present in.

Anxiety

Eisendrath et al. (2016) administered the State and Trait Anxiety Inventory (STAI) to their sample of people who were currently depressed and had not benefitted from previous treatment. Appearing to split the analysis of STAI scores by state and trait anxiety, the authors reported a non-specific predictive effect of state anxiety of a reduced treatment response rate (defined as a 50% or greater decrease from baseline HAM-D scores), and lesser reduction in depressive symptom severity (HAM-D scores) across both MBCT and the active control. Further, in their discussion, the authors state they found a treatment-independent, adverse predictive effect of trait anxiety on outcomes, but fail to report what these outcomes are and as noted above with other moderation analyses, do not report this in their results section.

As previously noted, this study's bias rating was found to be uncertain, and the quality rating for both state and trait anxiety moderation analyses was rated low (2-3). In conclusion, whilst study and moderation quality were questionable, the results are in line with the authors' link to the literature demonstrating adverse effects of anxiety on depression treatment outcomes in general (Ionescu, Niciu, Richards, & Zarate, 2014) and this would explain the same moderation effect being present in their Health Education Program (HEP) active control condition.

Physical Health

Eisendrath et al. (2016) report in their discussion that the presence of medical illness was evaluated for moderation effect with none found. No clarification was provided on what kinds of medical illness were included under this variable, and again, these authors failed to detail the analysis which led to this claim. The results of this moderation effect must therefore be interpreted with caution.

Table 1.

Characteristics and main findings of included studies

Author (year)	Sample	Clinical population(s)	Intervention	Control condition(s)	Length of follow-up	Moderator variable(s)	Main outcome variable(s)	Findings
Teasdale, Segal, Williams et al. (2000)	UK/Canada; n = 145; 54% women; 99% white	Participants currently in remission or recovery from MDD	MBCT (8 week standard)	TAU	12 months	(1) Recency of recovery from last depressive episode (DSM-III-R); (2) Number of previous episodes of MDD (DSM-III-R)	Depression relapse rate (DSM-III-R criteria)	The number of previous episodes of MDD statistically moderated relapse rate. In those with ≥ 3 previous episodes, MBCT was significantly more likely to protect from relapse than TAU. This effect was not present in those with 2 previous episodes. No effect of recency of previous depressive episode.
Ma & Teasdale (2004)	UK; n = 75; 76% women; 100% white	Participants in remission or recovery with ≥ 3 previous episodes of depression vs. 2 previous episodes	TAU + MBCT (8 week standard)	TAU	12 months (3- month interval s)	(1) Number of previous depressive episodes (DSM-III-R diagnosis); (2) Severity of previous episode	Depression relapse rate (DSM-IV diagnostic criteria)	Number of depressive episodes predicted the effect of MBCT on relapse rates. Those with less than 3 previous episodes were more likely to relapse following treatment, compared to those having experienced 3 or more.

Author (year)	Sample	Clinical population(s)	Intervention	Control condition(s)	Length of follow-up	Moderator variable(s)	Main outcome variable(s)	Findings
Crane & Williams (2010)	UK; n = 68; 62% women; ethnicity not reported	Participants in remission or recovery with ≥ 1 previous episode of MDD and a history of active suicidal ideation	MBCT (8-week standard + crisis plan development)	Wait-list (TAU)	N/A	(1) Age; (2) Gender; (3) Number of previous depressive episodes; (4) History of manic or hypomanic episodes (MINI); (5) Lifetime or current alcohol or substance abuse/dependence; (6) Presence of a borderline personality disorder diagnosis (SCID-II); (7) Antidepressant medication usage	Treatment dropout rate (those who attended <4 sessions)	Those who dropped out of MBCT were significantly younger than those who completed treatment, less likely to be on antidepressants, had higher levels of depressive rumination and brooding and showed significantly greater levels of problem-solving deterioration following mood challenge (cognitive reactivity). Gender, number of previous episodes of depression, a history of manic or hypomanic episodes, lifetime or current alcohol or substance abuse, or borderline personality disorder, were not found to predict outcomes.
Segal, Bieling, Young et al. (2010)	Canada; n = 160; 58% women; 80% white	Participants diagnosed with current MDD with ≥ 2 previous episodes	MBCT (8-week standard + monthly 1-hour classes) + medication taper	(1) M-ADM; (2) Medication taper + placebo + clinical management	18 months	(1) Number of previous depressive episodes (DSM-IV); (2) Quality of acute-phase remission (HAM-D)	Depression relapse rate (DSM-IV diagnostic criteria)	The quality of remission following antidepressant medication (stable vs. unstable) significantly predicted MBCT's effectiveness in reducing relapse. Unstable remitters were significantly less likely to relapse following MBCT (or M-ADM), compared to stable remitters. Number of previous depressive episodes significantly predicted relapse in MBCT and M-ADM.

Author (year)	Sample	Clinical population(s)	Intervention	Control condition(s)	Length of follow-up	Moderator variable(s)	Main outcome variable(s)	Findings
Kuyken, Watkins, Holden et al. (2010)	UK; n = 123; 77% women; 99% white	Participants in remission with ≥ 3 previous episodes of depression	MBCT (8 week standard) + medication taper	M-ADM	15 months	(1) Gender; (2) Age; (3) Marital status; (4) Religion; (5) Level of education; (6) Social class; (7) Depression remission quality (full vs. partial); (8) Presence of psychiatric diagnosis co-morbidity (DSM-IV); (9) Previous episodes of depression (DSM-IV); (10) Age of first depressive episode; (11) Baseline depression severity (HAM-D)	(1) Depressive symptom severity (HAM-D); (2) Depression relapse rate (DSM-IV)	Gender statistically moderated outcomes on the HAM-D at follow-up, with women demonstrating reduced levels of depression compared to men. No other baseline demographic or psychiatric variables were found to influence either outcome.
Van Aalderen et al. (2012)	Netherlands; n = 205; 71% women; ethnicity not reported	Participants currently in remission or currently depressed, with ≥ 3 previous episodes	TAU + MBCT (8-week standard + monthly 1-hour classes + silent days of consecutive groups)	TAU	12 months	Depression symptom severity (HAM-D + BDI)	Depression symptom severity (HAM-D + BDI)	Depression symptom severity was not found to predict post-MBCT levels of depression.

Author (year)	Sample	Clinical population(s)	Intervention	Control condition(s)	Length of follow-up	Moderator variable(s)	Main outcome variable(s)	Findings
Geschwind, Peeters, Huibers et al. (2012)	Netherlands; n = 130; 49% women; ethnicity not reported	Participants in remission of MDD with residual symptoms (≤ 2 previous episodes vs. ≥ 3 episodes)	TAU + MBCT (8-week standard with maximum group size of 15)	Wait-list (TAU)	12 months	Number of previous depressive episodes (SCID-I)	Depressive symptom severity (HAM-D + IDS)	No evidence was found to suggest that the number of previous depressive episodes influenced MBCT's effectiveness in reducing residual depressive symptoms.
Williams et al. (2014)	UK; n = 274; 72% women; 95% white	Participants in remission or recovery with ≥ 3 previous episodes of depression	MBCT (8-week standard + 6-month booster 2hr session)	TAU/CPE	12 months	(1) Childhood adversity (CTQ); (2) Residual depressive symptoms (BDI-II + HAM-D); (3) Age of first depressive episode; (4) Number of previous depressive episodes (SCID-I); (5) Antidepressant use	(1) Depression relapse rate (DSM-IV-TR diagnostic criteria)	MBCT was significantly more effective at preventing relapse for those who have experienced the greatest childhood adversity. Residual depressive symptoms statistically moderated outcomes across the conditions. Age of first depression onset, antidepressant use at baseline, and number of previous depressive episodes, did not moderate relapse rates.

Author (year)	Sample	Clinical population(s)	Intervention	Control condition(s)	Length of follow-up	Moderator variable(s)	Main outcome variable(s)	Findings
Huijbers et al. (2016)	Netherlands; n = 317; 69% women; ethnicity not reported	Participants in full or partial remission, with ≥3 previous episodes of depression	MBCT (8-week standard + 3 booster sessions) + M-ADM	MBCT alone vs. M-ADM alone	15 months (3-month interval)	(1) Preference for MBCT or M-ADM; (2) MBCT preference strength (TCQ - Modified)	(1) Number of MBCT sessions attended; (2) Amount of home MBCT practice reported; (3) Depression relapse rate (SCID-I)	MBCT preference strength did not predict the number of sessions attended, the amount of home practice reported, or the time to depression relapse. Preference for either MBCT or M-ADM did not predict time to depression relapse.
Bakker et al. (2016)	Netherlands; n = 129; 69% women; ethnicity not reported	Participants with residual depressive symptoms, following ≥1 episode of MDD	MBCT (format not reported)	Wait-list	Unknown	Antidepressant medication usage	(1) PA (unknown measure); (2) NA (unknown measure)	MBCT was found to significantly decrease NA for those taking antidepressant medication, compared to those not taking any. Conversely, post-MBCT increases in PA scores were found to be significantly greater for those not taking antidepressants, compared to those who were.

Author (year)	Sample	Clinical population(s)	Intervention	Control condition(s)	Length of follow-up	Moderator variable(s)	Main outcome variable(s)	Findings
Eisendrath et al. (2016)	US; n = 173; 76% women; 80% white	Participants diagnosed with current treatment resistant MDD + 2 failed antidepressant drug trials/ ≥ 1 episode of depression	MBCT (8 week modified: shortened practice (30min); emphasised mindful movement; exploring barriers to practice; incorporating a focus on acceptance of emotional events)	HEP	12 months	(1) Childhood adversity (CTQ); (2) Anxiety (STAI); (3) Current stress (PSS); (4) Presence of a personality disorder diagnosis (SCID-II) (5) Socioeconomic status; (6) Gender; (7) Number of previous depressive episodes (SCID-I); (8) Age of first depressive episode; (9) Depression severity (HAM-D); (10) Education level; (11) Disability status (physical or mental); (12) Presence of medical illness	(1) Depressive symptom severity (HAM-D); (2) Depression remission rate (scores after treatment ≤ 7 on HAM-D); (3) Treatment response ($\geq 50\%$ decrease from baseline HAM- D scores)	Stress and personality disorder presence were found to reduce MBCT and HEP's effectiveness in reducing depressive symptom severity. No statistical moderation found by any other variable on all outcome measures.

Author (year)	Sample	Clinical population(s)	Intervention	Control condition(s)	Length of follow-up	Moderator variable(s)	Main outcome variable(s)	Findings
Michalak, Probst, Heidenreich et al. (2016)	Germany; n = 106; 62% women; ethnicity not reported	(1) Chronic MDD; (2) Double depression; (3) Current MDD as part of a recurrent MDD with incomplete recovery between episodes during the last 2 years	TAU + MBCT (8 week modified: maximum of 6 per group)	(1) CBASP + TAU; (2) TAU	6 months	Childhood adversity (CTQ)	Depressive symptom severity (HAM-D and BDI-II)	At 6-month follow-up, childhood adversity statistically moderated HAM-D scores, but not BDI-II scores. The greater the history of adversity, the greater the reduction in HAM-D scores.

Note. MDD = Major Depressive Disorder; CBASP = Cognitive Behavioural Analysis System of Psychotherapy; CPE = Cognitive Psychological Education; HEP = Health Enhancement Programme; MBCT = Mindfulness-based Cognitive Therapy; M-ADM = Maintenance antidepressant medication; TAU = Treatment as Usual; BDI = Beck Depression Inventory; BDI-II = Beck Depression Inventory Second Edition; CTQ = Childhood Trauma Questionnaire; DSM-III-R = Diagnostic and Statistical Manual of Mental Disorders, Third Edition-Revised; DSM-IV = Diagnostic and Statistical Manual of Mental Disorders, Fourth Edition; DSM-IV-TR = Diagnostic and Statistical Manual of Mental Disorders, Fourth Edition, Text Revision; HAM-D = Hamilton Rating Scale for Depression; IDS = Inventory of Depressive Symptoms; MINI = Mini International Neuropsychiatric Interview; NA = Negative affect; PA = Positive affect; PSS = Perceived Stress Scale; SCID-I = Structured Clinical Interview for DSM-IV Axis I Disorders; SCID-II = Structured Clinical Interview for DSM-IV Axis II Disorders; STAI = State and Trait Anxiety Inventory; TCQ = Treatment Credibility Questionnaire.

Antidepressant Medication

Two studies looked at whether taking ADM at baseline affected participant outcomes following MBCT. Williams et al. (2014) reported that medication use did not influence relapse rates of depression, and this moderation analysis was rated as medium quality (5). Bakker et al. (2016), on the other hand, uncovered a more nuanced finding with their population of participants with one or more previous depressive episodes. These authors reported that ADM appeared to act as an adjunct to MBCT by significantly reducing negative affect (NA) when compared to those not taking ADM. However, for positive affect (PA), MBCT produced greater increases in those not taking ADM. These findings were explained by the authors who cited evidence for ADM's dampening effect on brain regions associated with both NA and PA (Y. Ma, 2015). Whilst affect is not a direct measure of depression, increases in PA and decreases in NA have consistently been linked to psychotherapeutic interventions for adults with depression (for a recent meta-analysis, see: Boumparis, Karyotaki, Kleiboer, Hofmann, & Cuijpers, 2016).

The Bakker et al. (2016) paper received a high risk of bias rating due to a lack of clarity around multiple possible influences of bias, and a medium quality rating for its moderation analysis (4). However, their use of affect as an outcome may represent a more finely-grained look into the interaction between MBCT and ADM when compared to the relatively coarser outcome of rate of relapse to depression used by Williams et al. (2014). Further research is needed to further investigate the impact of ADM use on MBCT outcomes, and affect may be a useful complementary outcome to include.

Stress

Eisendrath et al. (2016) found that the presence of increased stress at baseline, as measured by the Perceived Stress Scale (PSS), predicted lesser improvement on post-

intervention depressive symptom severity on the HAM-D, although this was not exclusive to MBCT. Further, the reader is reminded of the relatively poor quality of this study and its moderation analyses.

Presence of a Personality Disorder Diagnosis

This negative, non-specific effect on the HAM-D was also significantly related to the presence of a personality disorder (Eisendrath et al., 2016).

Presence of Psychiatric Comorbidity

A psychiatric co-morbidity was not found to moderate depressive symptom severity or rate of relapse of depression in Kuyken et al.'s (2010) study, with an uncertain bias rating.

Treatment preference

Huijbers, Spinhoven, van Schaik, Nolen, and Speckens (2016) conducted a large-scale ($n = 317$) trial comparing MBCT plus M-ADM, MBCT alone, or M-ADM alone, administered to participants with 3+ previous episodes in remission or recovery. The authors reported that neither the preference for psychological (MBCT) versus medical (M-ADM) treatment for depression, nor the strength of that preference, predicted: the number of sessions of MBCT attended; the amount of home MBCT practice reported; or rate of relapse to depression.

The moderation analyses reported were rated as high quality (6-7). However, the paper presents an uncertain risk of bias due to lack of clarity around allocation concealment and whether outcome assessors were blinded to the treatment condition a participant received. This uncertainty around bias, in addition to the fact that the authors excluded those participants with a definite preference for either MBCT or M-ADM (as this was incompatible with their randomisation procedure), questions the reliability of these findings. The exclusion of those with a definite preference also limits the generalisability of this finding, as their sample may

represent a population of participants which does not represent those seen in real-world clinical settings.

Sociodemographics

Age. An additional characteristic of the 3+ previous depressive episodes population, suggested by Ma and Teasdale (2004), was an older age at trial entry when compared to the two or fewer previous episode population. Kuyken et al. (2010) did not find any evidence to suggest that age moderated depressive symptom severity or relapse to depression following MBCT in their 3+ episode sample. As was previously suggested regarding the moderation effect of number of previous episodes on relapse rate of depression, it might be that by only including the 3+ population in Kuyken et al. (2010), no effect of age on outcomes was detected.

Gender. Reporting on the gender of participants, Kuyken et al. (2010) found, at 15-month follow-up, women demonstrated significantly lower levels of depressive symptom severity (HAM-D) and a trend towards reduced rates of relapse to depression, compared to men post-MBCT. The authors also added that archive data was obtained from the Teasdale et al. (2000) and Ma and Teasdale (2004) studies, which did not replicate their own gender moderation effect. The analysis by Eisendrath et al. (2016) also failed to find any moderation effect of gender on study outcomes. Kuyken et al.'s (2010) finding could reflect a type I error due to the authors having conducted many moderation analyses. However, when considering that this gender effect was the only significant statistical moderation detected across both outcomes, it would be prudent to investigate this further prior to discounting gender as a moderator.

Table 2

Risk of bias of included studies using a modified Cochrane Risk of Bias II tool

Author (year)	Sequence generation	Allocation concealment	Blinding - outcome assessors	Intention-to-treat analysis	Treatment fidelity assessed	Participant adherence		
						to intervention	Planned analysis	Risk of bias
Teasdale, Segal, Williams et al. (2000)	?	+	?	+	+	-	-	Low
Ma & Teasdale (2004)	+	+	?	+	+	-	-	Low
Crane & Williams (2010)	?	+	?	-	-	-	-	Low
Segal, Bieling, Young et al. (2010)	+	+	+	+	+	-	?	Low
Kuyken, Watkins, Holden et al. (2010)	+	?	+	+	+	-	-	Uncertain
Geschwind, Peeters, Huibers et al. (2012)	+	+	-	+	-	+	?	Low
Van Aalderen et al. (2012)	+	+	?	+	?	+	?	Low
Williams, Crane, Barnhofer et al. (2014)	+	+	+	-	+	+	+	Low
Bakker et al. (2016)	?	?	?	?	?	?	?	High
Huijbers et al. (2016)	+	?	?	+	+	+	+	Uncertain
Eisendrath et al. (2016)	+	?	+	+	+	-	?	Uncertain
Michalak, Probst, Heidenreich et al. (2016)	+	+	+	+	+	?	?	Low

Note. + = criterion met; ? = unclear whether criterion met; - = criterion not met.

Level of education. Kuyken et al. (2010) and Eisendrath et al. (2016) also entered participants' level of education into their moderation models, with both reporting a null effect on outcomes. Eisendrath et al. (2016) again did not report their analyses and received the lowest moderation analysis quality scoring (0).

Mental or physical disability. Eisendrath et al. (2016) report no moderation effect of the presence of a psychiatric or physical disability at baseline, although this analysis is not reported in detail and is not clearly operationalised.

Religion. Kuyken et al. (2010) report no evidence that religion moderates MBCT's outcomes on HAM-D scores or rate of relapse to depression following MBCT. However, over 75% of participants in this study were Christian, limiting the extent to which this moderator was tested and the generalisability of the findings.

Marital status. No impact of marital status on MBCT outcomes was demonstrated by Kuyken et al. (2010), although less than 20% of their sample were single, compared to 68% married or co-habiting, skewing representativeness.

Social class. Kuyken et al. (2010) also entered social class as a predictor into their analyses and found no effect of moderation on outcomes. However, their sample was overrepresented by those in managerial and professional occupations.

Socioeconomic status. Eisendrath et al. (2016) found no effect for the socioeconomic status of their participants, although this analysis, or the classification definition used for this variable, were not reported meaning that this finding should be interpreted with caution.

Table 3

Moderator analyses from included studies and their quality scoring

Author/Year	Moderator category	Moderator variable	Valid and reliable moderator measure	Moderator tested pre-randomisation	<5 moderator s tested	A-priori hypothesis	Direct test of interaction	Valid and reliable outcome measure	Planned moderator analysis	Total quality score
Childhood adversity										
Williams, Crane, Barnhofer et al. (2014)			1	1	0	1	1	1	1	6
Eisendrath et al. (2016)			1	?	0	1	0	1	1	4
Michalak et al. (2016)			1	?	1	0	1	1	0	4
Mood disorder										
Kuyken, Watkins, Holden et al (2010)		Age of first episode	1	1	0	0	1	1	0	4
Eisendrath et al. (2016)			?	?	0	1	?	?	1	2
Williams, Crane, Barnhofer et al. (2014)			1	1	0	1	1	1	1	6
Teasdale et al. (2000)		Number of previous episodes	1	1	1	1	1	1	0	6
Ma & Teasdale (2004)			1	1	0	1	1	1	1	6
Segal, Bieling, Young et al. (2010)			1	1	1	0	1	1	0	5
Kuyken, Watkins, Holden et al (2010)			1	1	0	0	1	1	0	4

Geschwind, Peeters, Huibers et al. (2012)			1	1	1	1	0	1	1	6
Williams, Crane, Barnhofer et al. (2014)			1	1	0	1	1	1	1	6
Eisendrath et al. (2016)			?	?	0	1	?	?	1	2
Recency of previous episode										
Teasdale et al. (2000)			1	1	1	0	1	1	0	5
Severity of previous episode										
Ma & Teasdale (2004)			1	1	0	0	0	1	0	3
Baseline depression severity										
Segal, Bieling, Young et al. (2010)			1	1	1	1	1	1	1	7
Kuyken, Watkins, Holden et al (2010)	(Categorical)		1	1	0	0	1	1	0	4
Kuyken, Watkins, Holden et al (2010)	(Continuous)		1	1	0	0	1	1	0	4
Williams, Crane, Barnhofer et al. (2014)			1	1	0	1	1	1	1	6
Van Aalderen et al. (2012)	(Continuous)		1	1	0	0	1	1	1	5
Van Aalderen et al. (2012)	(Categorical)		1	1	0	1	1	1	1	6
Eisendrath et al. (2016)			?	?	0	0	?	?	0	0
Anxiety										
Eisendrath et al. (2016)		Trait anxiety	1	?	0	0	?	?	1	2
Eisendrath et al. (2016)		State anxiety	1	?	0	0	0	1	1	3

Sociodemographics									
Kuyken, Watkins, Holden et al (2010)	Age	1	1	0	0	1	1	0	4
Kuyken, Watkins, Holden et al (2010)	Gender	1	1	0	0	1	1	0	4
Eisendrath et al. (2016)		1	1	0	0	0	1	0	3
Kuyken, Watkins, Holden et al (2010)	Education level	1	1	0	0	1	1	0	4
Eisendrath et al. (2016)		?	?	0	0	?	?	0	0
Eisendrath et al. (2016)	Disability status (physical or psychiatric)	?	?	0	0	?	?	1	1
Kuyken, Watkins, Holden et al (2010)	Religion	1	1	0	0	1	1	0	4
Kuyken, Watkins, Holden et al (2010)	Marital status	1	1	0	0	1	1	0	4
Kuyken, Watkins, Holden et al (2010)	Social class	1	1	0	0	1	1	0	4
Eisendrath et al. (2016)	Socioeconomic status	?	?	0	1	?	?	1	?
Physical health and medication									
Bakker et al. (2016)	Use of antidepressant medication	1	?	?	1	1	?	1	4
Williams et al. (2014)		1	1	0	0	1	1	1	5
Eisendrath et al. (2016)	Presence of medical illness	?	?	0	0	?	?	?	0

General psychological factors									
Eisendrath et al. (2016)	Stress	1	?	0	1	0	1	1	4
Kuyken, Watkins, Holden et al (2010)	Presence of psychiatric co-morbidity	1	1	0	0	1	1	0	4
Eisendrath et al. (2016)	Presence of a personality disorder diagnosis	1	?	0	1	0	1	1	4
Treatment preferences									
Huijbers et al. (2016)	Treatment preference strength	?	1	1	1	1	1	1	6
Huijbers et al. (2016)	Treatment preference (ADM vs psych/mindfulness)	1	1	1	1	1	1	1	7

Note. 0 = criterion not met; 1 = criterion met; ? = unclear whether criterion met; total score represents a sum of all of the criteria being met scores (1s).

Summary. Two studies conducted multiple planned and exploratory moderation analyses, entering various sociodemographic variables. Gender was the sole variable reported as a predictor of depressive symptom severity post-MBCT, with women showing greater reductions, although other studies failed to find such an effect and the possibility of this finding resulting from a Type I error was noted. No other effects of moderation were identified, although this should not be treated as definitive due to the limited number of studies, the mixed study bias ratings and quality of moderation analyses.

MBCT Attrition as an Outcome Measure

Whereas the studies outlined above included primary outcome measures relating to depression or mood (i.e. Bakker et al., 2016), one paper returned from the literature search chose to focus instead on treatment dropout rate. Due to this difference, this study is described separately here. Crane and Williams (2010), reporting on their earlier RCT (Crane et al., 2008), were interested in identifying which factors might impact the likelihood that their sample of participants with one or more previous depressive episode, in recovery or remission and with a history of suicidal ideation or behaviour, would drop out of MBCT (defined as attending less than four sessions). Overall, this study received a low risk of bias rating, although it was unclear as to whether outcome assessors were blinded to participant condition.

The authors reported that those who scored lower on cognitive reactivity, and brooded less at baseline, were statistically less likely to drop out of MBCT. Cognitive reactivity was defined as the ability to problem-solve effectively following negative mood induction, and brooding was a sub-scale of rumination.

Null results were reported on the following variables, in terms of moderating the dropout rate of those randomised to MBCT: number of previous depressive episodes; presence

of a history of manic or hypomanic episodes; current or lifetime alcohol/substance abuse or dependence; gender; and presence of a personality disorder diagnosis.

Moderation analysis quality ranged from low to medium (2-5), mainly due to a large number of variables being entered into moderation analysis, lack of clarity around what form of moderation analysis was used, and the post-hoc nature of the paper.

Summary. This paper presents findings which may be of value when considering which participant characteristics might indicate the likelihood of disengagement from MBCT. Increased cognitive reactivity and brooding were found to independently predict drop-out, and as such, investigating ways in which to modify MBCT to make it more acceptable to people measuring highly in these areas at baseline would be of great benefit, although replication of these results using clearly planned and defined moderation analyses in larger samples, and including participants without suicidality is also important.

Discussion

This review aimed to systematically search the literature to identify whether person-level variables exist which moderate the outcomes of MBCT for depression. Twelve articles were included which examined a range of potential moderator variables across multiple different outcomes, of which relapse rate to depression and severity of depressive symptoms were the most commonly studied. The risk of bias presented by the articles was rated as generally low with some uncertainty generally resulting from lack of clarity in study reporting, although the quality of moderation analyses was highly varied. Together, the evidence-base suggests that there are several person-level variables which predict (or moderate) outcomes of MBCT for depression and the implications of this are discussed below.

The number of previous depressive episodes was entered into moderation analyses across seven studies and there was reasonable evidence to suggest that this moderated the rate at which people experienced relapse of depression following MBCT, where only people having experienced three or more previous depressive episodes demonstrated a reduced rate. However, it was noted that there was risk of bias in the Kuyken et al. (2010) and Eisendrath et al. (2016) papers, in addition to a relatively lower quality in their moderation analyses (4 and 2, respectively), meaning further replication of this moderation effect is necessary. Further, this effect was only found in studies which included people having experienced two or more previous depressive episodes as opposed to 3+ episodes only (the NICE guidance eligibility criteria for MBCT) and fits with the suggestion of Ma and Teasdale (2004) that people with a history of 3+ depressive episodes represent a distinct clinical population which benefit from MBCT to a significantly greater extent. However, this moderation effect was not specific to MBCT, instead also being observed in those taking M-ADM which suggests that there may exist some characteristic(s) of this hypothesised sub-population which enable them to be more responsive to therapeutic input in general. Further research should seek to clarify whether these findings do indeed reflect the presence of separate populations of patients, and if so, what defines them. Ma and Teasdale (2004) made some observations of differences in their sample which may begin to answer this latter point, in that the 3+ participants had experienced their first depressive episode at an earlier age, they had experienced greater childhood adversity, and were older on average.

This moderation of rate of relapse by the number of previous depressive episodes was then contrasted with findings that this variable does not appear to moderate the outcome of depressive symptom severity, although caution was urged in interpreting these findings due to the risk of bias in Kuyken et al. (2010) Eisendrath et al. (2016), and their medium to low quality moderation analyses scores (4 and 2, respectively). It is possible that the participants with two

previous episodes in two of these trials (Eisendrath et al., 2016; Geschwind et al., 2012) were from the same population (at an earlier stage of their depressive “career”) as those presenting with 3+ previous episodes and therefore benefitted from MBCT to the same extent in terms of depressive symptom reduction. If this were the case, this subgroup would also be predicted to demonstrate equivalent reductions in relapse rates following MBCT. Unfortunately, relapse rate was not an outcome of these studies so this cannot be determined. An alternative interpretation is that changes in relapse rate are dissociable from changes in depressive symptom severity. This would reflect the effectiveness of MBCT on depressive symptoms in populations of non-recurrently depressed participants (e.g. Tovote et al., 2017; Williams et al., 2008). Further, even if this two-episode population did then go on to relapse, it is possible that MBCT would have equipped them with the skills to cope with this more effectively than if they had not received the intervention. Research could test this by following up such a group of participants and measuring the severity and duration of a subsequent depressive relapse, where shorter and less severe episodes would be predicted. Regardless, these findings indicate that the current NICE guidance (2009) of offering MBCT to only those with 3+ previous episodes may be overly prohibitive due to its focus only on relapse rate as an outcome. MBCT has the potential to offer equally cost-effective treatment for depression compared to M-ADM (Kuyken et al., 2015), and further research should seek to replicate the above findings with a view to lobby the broadening of the NICE guideline eligibility criteria.

Regarding characteristics of previous depressive episodes such as the recency of a person’s last episode, that episode’s severity, or the age a person first experienced depression, no moderation effect was found across five studies on either the rate of relapse to depression or the severity of depressive symptoms following MBCT. The hypothesised separate 3+ episode clinical population described by Ma and Teasdale (2004) was defined as having

experienced their first depressive episode at a younger age, but this review's findings may therefore imply that this is not a reliable indicator of such a population.

The results of the five studies which included baseline depression symptom severity in moderation analyses were mixed. 'Unstable' remitters following a course of antidepressant medication were found to be significantly less likely to relapse than those not experiencing depressive symptom flurries following MBCT or M-ADM (Segal et al., 2010), and those scoring higher on the HAM-D at baseline were at greater risk of relapse across all conditions in the study by Williams et al. (2014). Both of these studies were rated as presenting a low risk of bias and high-quality moderation analyses. The additional three studies (Eisendrath et al., 2016; Kuyken et al., 2010; van Aalderen et al., 2012) found no effect of baseline depression symptom severity on either rate of relapse of depression or symptom severity post-intervention. The differences in the classification of symptom severity and the number of depressive episodes experienced by participants made comparing these findings difficult. However, two of the studies included participants whose baseline depressive symptom severity was great enough for them to classify as currently depressed (van Aalderen et al., 2012) or only partially remitted (Kuyken et al., 2010). Importantly, whilst caution should be taken in generalising the failure of these studies to find a moderation effect of depression symptom severity due to the uncertain risk of bias in Eisendrath et al. (2016) and Kuyken et al. (2010), and their very low to medium quality moderation analyses, this result can tentatively be taken to further the argument made above: that in restricting the recommendation of MBCT to those currently in remission from depression, the current NICE guidelines (NICE, 2009) are preventing those people currently depressed or not achieving remission from accessing a therapeutic intervention which is likely to benefit them.

The amount of childhood adversity experienced by MBCT participants was entered as a moderator in three studies' analyses following the observation by Ma and Teasdale (2004)

that increased childhood adversity was a common feature of their 3+ episode population. The results of two of these studies suggest that for those who reported greater childhood adversity, outcomes following MBCT are significantly better in terms of reduced relapse rate (Williams et al., 2014), and reduced HAM-D depressive severity at 6-month follow-up (Michalak et al., 2016). Whilst the third study (Eisendrath et al., 2016) did not find childhood adversity to moderate MBCT outcomes, this may be attributable to its increased risk of bias and relatively lower moderation quality compared to Williams et al. (2014). These results therefore remain important and warrant further replication because childhood adversity is known to increase the risk of depression and the likelihood that participants will experience poor treatment outcomes (Randolph & Dykman, 1998; Sakado, Sato, Uehara, Sakado, & Someya, 1999). Therefore, MBCT's indicated increased effectiveness for people presenting with such a history may mean it is a highly suitable approach. Supportive evidence for this comes from research demonstrating that children and young adults who have been exposed to childhood trauma and stress, tolerate and benefit from a mindfulness-based therapeutic approach (Ortiz, Sibinga, Ortiz, & Sibinga, 2017).

Eisendrath et al. (2016) reported that increased levels of stress at baseline were predictive of post-intervention depressive symptom severity, regardless of which intervention participants received. In addition, they reported an adverse effect of anxiety at baseline on HAM-D score reduction post-treatment (across MBCT and control), which supported previous research demonstrating deleterious effects of anxiety on depression treatment outcomes (Ionescu et al., 2014). Further, these authors reported that participants with a personality disorder diagnosis saw less improvement on the HAM-D across treatment conditions, and further research with this client group should focus on whether interventions could be adapted to improve this outcome. However, this study's poor clarity around risks to bias and generally low-quality moderation analyses temper the conclusions that can be drawn from these findings.

Consideration of the effect of participants taking antidepressant medication at baseline was given by two studies (Bakker et al., 2016; Williams et al., 2014) which produced conflicting results, with Williams et al. (2014) finding no moderation effect on relapse rate, and Bakker et al. (2016) showing a moderation effect on positive and negative affect. This conflict may be explained by the high risk of bias present in Bakker et al.'s (2016) study. However, an alternative explanation lies in the difference in outcome measure used, where affect is arguably more sensitive than relapse rate. If the moderation effect in Bakker et al.'s (2016) study is replicable, it might suggest that for those taking ADM (who therefore see less increase post-MBCT on positive affect), targeted adjunctive intervention to increase positive affect could improve MBCT outcomes further as increases in positive affect have been demonstrated to facilitate upward spirals of emotional well-being which may otherwise be precluded in this group (Fredrickson & Joiner, 2002).

A variety of other variables, including treatment preference, sociodemographics, and the presence of psychiatric comorbidity, which were entered into moderator analyses across the reviewed studies failed to demonstrate any effect on study outcomes. Whilst this might suggest that these variables do not influence MBCT outcomes, it is important that further research replicates such null effects, particularly as in the present context they were often entered alongside large numbers of other potential moderator variables (e.g. Eisendrath et al., 2016) and as such the null result may in fact reflect Type II errors missing genuine moderation effects due to a lack to experimental power.

Finally, one study was included which focused on a different outcome to the others which was MBCT drop-out rate (Crane & Williams, 2010). These authors found no moderation of drop-out rate by the number of previous depressive episodes, the presence of a history of manic or hypomanic episodes, current or lifetime alcohol or substance abuse, gender, or a diagnosis of personality disorder. However, increased cognitive reactivity to low mood and

increased brooding at baseline were predictive of significantly greater drop-out, and the authors suggest that people with this presentation are paradoxically most likely to benefit if they do complete MBCT. This fits with MBCT theory on reduction of rumination (of which brooding is an indicator) by teaching participants to disengage from the ‘driven-doing’ mode of mind and engage with the ‘being’ mode (Segal et al., 2002), where presumably those who brood more have a greater capacity for positive change away from this thinking style if they remained engaged. Further, mediation research has confirmed the key roles of reducing rumination and cognitive reactivity in MBCT mental health outcomes (Gu, Strauss, Bond, & Cavanagh, 2015). It is therefore of great importance for research to determine how cognitive reactivity and brooding can be assessed for prior to MBCT and how the approach might be altered to accommodate for these factors. One solution might be to have a “gentler” approach with shorter meditation times and a longer course, to allow participants to ease into turning towards and tolerating the more readily provoked depressive thinking styles resulting from their increased cognitive reactivity and tendency to brood.

Clinical and Research Implications

The findings that the number of previous depressive episodes did not statistically moderate depressive symptom severity post-MBCT, or that whether a person is currently depressed or in remission does not appear to affect the rate of relapse to depression or depressive symptom severity post-MBCT, indicate that the current NICE guidance (2009) recommending MBCT only for those with three or more previous episodes, currently in remission, is likely to be resulting in people who could benefit from the intervention being denied access. Should further research replicate these findings, it is crucial that these guidelines are changed to allow wider access to MBCT.

The preliminary evidence provided by two studies that MBCT appears more effective in reducing relapse rates and depression severity in those with the greatest levels of childhood adversity is of significant clinical relevance due to the documented poor outcomes for depression from alternative interventions in this population (e.g. Randolph & Dykman, 1998; Sakado et al., 1999). Further research is required replicating this effect, in addition to investigating whether these participants are able to tolerate completion of the intervention to ensure that MBCT would be an effective treatment in the clinical setting. Research demonstrates that those who have experienced childhood adversity are less reactive to stress (Lovallo, 2013), which may complement the findings of Crane and Williams (2010) where those low on cognitive reactivity were more likely to complete MBCT.

Future research should aim to provide greater transparency around whether outcome assessors were blinded, to increase confidence in the absence of study bias. Further, it should aim to take a more theory-driven and focused approach to selection and analyses of potential moderators to reduce the possibility of Type II errors arising from underpowered, explorative, multi-comparison approaches (for example, in Eisendrath et al., 2016), and to enable a-priori hypotheses of moderation effects to be made in order to improve the validity of reported effects. Finally, due to the significant cost of RCTs, one possible approach to testing such theory-driven hypotheses of moderation effects which would save time and money could be to use a brief experimental mindfulness meditation paradigm as an initial piloting format. Brief mindfulness meditations as short as 10-minutes have demonstrated changes in state affect, and mindfulness (Frieze, Messner, & Schaffner, 2012; Hopthrow, Hooper, Mahmood, Meier, & Weger, 2017; Slater, Strauss, & Hayward, 2014; Weger, Hooper, Meier, & Hopthrow, 2012), which could be treated as analogous to outcomes of MBCT, where reliable changes in affect following treatment for depression have been documented (Boumparis et al., 2016).

Conclusion

This review found evidence of varying strength that several person-level variables exist which have the potential to impact upon, or moderate different outcomes of MBCT for depression. These variables were the number of previous episodes of depression, the severity of baseline depressive symptoms, childhood adversity, anxiety, stress, antidepressant medication usage, the presence of a personality disorder diagnosis, gender, brooding and cognitive reactivity. No evidence of a moderating effect was found for physical health problems, select characteristics of previous depressive episodes, psychiatric comorbidity, age, level of education, mental or physical disability, religion, marital status, social class or socioeconomic class. Further research is required to replicate these findings in addition to identifying other variables which moderate MBCT outcomes, and to investigate ways in which the intervention may be adapted in order to better-suit those identified as likely to experience worse outcomes.

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Section B: Empirical Paper

Adapting brief mindfulness meditation to the individual: Consideration of trait
self-criticism

Word Count: 8000 (8248)

Abstract

Mindfulness-based Cognitive Therapy's (MBCT) application has become increasingly widespread following its establishment as an effective treatment for depression relapse prevention. Mindfulness meditation (MM) is an integral component of MBCT, but little research has looked at MM guidance instructions themselves. This study aimed to compare the efficacy of two types of MM instruction in improving state affect, in addition to examining whether the type of instruction interacted with the trait characteristic of self-criticism to influence positive affect specifically. Seventy participants were randomised to groups doing either a concentration-oriented or an acceptance-oriented 10-minute MM of the breath. Changes between the groups in pre- to post-meditation state affect, state mindfulness and state concentration were analysed using a three-way mixed ANOVA, with baseline self-criticism entered as a between-participant variable to investigate its hypothesised interaction (or, moderation) with meditation type and outcomes across time.

Significant improvements in mindfulness, concentration and negative affect were seen across both meditations, with the concentration-oriented meditation improving concentration significantly more than the acceptance-oriented condition. Further, those categorised as highly self-critical demonstrated a significant reduction in negative affect compared to no reduction for low self-critical participants. The hypothesised interaction between self-criticism and meditation type was not found. Modifying MM of the breath instructions may hold potential in selectively targeting outcomes such as concentration, and self-criticism may predict whether non-clinical participants benefit from improved mood following either form of brief MM. The study's findings are discussed in addition to identifying limitations, clinical implications and directions for future research.

Key words: mindfulness, meditation, moderation, self-criticism

Introduction

Mindfulness-based Cognitive Therapy (MBCT) has been demonstrated to be a reliable and effective intervention for reducing the risk of relapse in those currently in remission from a diagnosis of recurrent major depressive disorder (MDD), who have experienced three or more previous depressive episodes (Piet & Hougaard, 2011). MBCT is currently recommended by the National Institute for Health and Care Excellence for this clinical population (NICE, 2009). Further, some evidence has begun to emerge suggesting that MBCT may also be beneficial in reducing depressive symptom severity for those in remission with a history of fewer than three episodes of MDD (Geschwind et al., 2012), and for those currently diagnosed with MDD (Finucane & Mercer, 2006; van Aalderen et al., 2012a; van Aalderen, Donders, Peffer, & Speckens, 2015).

Whilst being initially developed as a treatment for MDD, MBCT's application has gradually widened, demonstrating promise in symptom reduction for other clinical populations; for example, in reducing the experience of persistent pain in women being treated for breast cancer (Johannsen, O'Connor, O'Toole, Jensen, & Zachariae, 2018); reducing depressive symptoms in diabetics (Tovote et al., 2017); and for reducing both residual depressive symptoms and severity of anxiety in people diagnosed with bipolar disorder currently in remission (J M G Williams et al., 2008).

Further, MBCT has begun to be applied to non-clinical populations such as undergraduate university students, with whom it has been found to significantly reduce depressive and anxious feelings prior to, during and following stressful exams (Kaviani, Javaheri, & Hatami, 2011). In addition, one study demonstrated that MBCT significantly improved NHS staff wellbeing, resulting in reduced staff sickness and financial savings for the

trust involved (Graham, 2014). Another found improvements in staff perceived stress and self-compassion (Marx, Strauss, & Williamson, 2014).

Segal, Williams and Teasdale (2012) describe a conceptual framework for understanding MBCT's mechanism of action. They posit the existence of a number of core 'modes' of mind, with the 'being' and 'doing' modes argued to be of central importance to MBCT. The 'doing' mode is oriented towards achieving both internal and external goals and utilises a discrepancy monitoring process in order to monitor goal progress. Segal and colleagues (2012) argue that the continued application of this mode towards internal goals (termed the 'driven-doing' mode) - noticing the discrepancy between current and hoped-for states and unsuccessfully attempting to reduce the discrepancy - drives the process of rumination. Rumination is well-established in the causation and maintenance of depression (Crane & Williams, 2010; Segal et al., 2012; Spasojevic & Alloy, 2001; Spasojevic, Alloy, Abramson, Maccoon, & Robinson, 2004; Treynor et al., 2003) and reducing rumination is one of the key mechanisms through which MBCT appears to have its beneficial effects on mental health (Gu et al., 2015).

MBCT involves training to increase awareness of which of these modes are currently operating, in addition to developing a non-judgmental, accepting attitude and present-moment focused attention, which are thought to facilitate disengagement from the 'driven-doing' mode, and switching into the 'being' mode. The 'being' mode involves acceptance of present moment experience, whatever this may be, and disengagement from the discrepancy-based processing characterising the 'driven-doing' mode. In the spiritual tradition of Buddhist mindfulness meditation (MM), which MBCT draws heavily upon (Segal et al., 2012), an approach frequently used is that of samatha which is a form of MM said to bring about mental calm and steadiness through an emphasis on the cultivation of concentration, usually on the breath

(Yates, Immergut, & Graves, 2017). Therefore, samatha, or concentration-oriented MM, can be seen to correspond to the component of MBCT involving the development of present-moment focused attention. This form of MM can be contrasted with acceptance-based MM which emphasises the acceptance of all moment-to-moment experience, without judgement or attempting to change it (Ainsworth, Bolderston, & Garner, 2017), and can thusly be seen to correspond to the development of a non-judgemental and accepting attitude in MBCT as described above.

It is likely that a range of differences exist in people's tendencies towards engaging in either the 'being' or 'driven-doing' modes, and that certain characteristics or personality traits might influence this. Self-criticism is one variable which is significantly associated with a tendency to ruminate (Manfredi et al., 2016; Spasojevic & Alloy, 2001; Spasojevic et al., 2004), playing a maintaining role in depression, and a tendency towards self-criticism is highly predictive of depressive symptoms (Gilbert, Clarke, Hempel, Miles, & Irons, 2004; Manfredi et al., 2016). Self-criticism has been defined as both a state (Blatt, Quinlan, Chevron, McDonald, & Zuroff, 1982) and a trait (Zuroff, Sadikaj, Kelly, & Leybman, 2016), but no consensus has been reached in the literature regarding which may be a more valid conceptualisation (Fritzsche, 2016). It may be more useful to conceptualise self-criticism as both a state and a trait, varying on a continuum from person to person depending on both a person's general tendency to be self-critical and the internal and external context in which a they find themselves. However, for the purposes of this study, it was treated as a relatively stable trait-like characteristic in order to examine individual differences in the hypothesised general tendency towards criticising oneself.

The increased tendency to ruminate in people with a greater tendency towards self-criticism (or, high levels of trait self-criticism) may therefore also suggest increased

engagement in discrepancy monitoring, and thus being in a ‘driven-doing’ mode more of the time. Whilst it follows that in groups of people completing MBCT for MDD, the mean level of trait self-criticism would be expected to be high, in the general population a more varied range of self-criticism would be expected. Indeed, normative data on the Forms of Self-criticising/Attacking and Self-Reassuring Scale (FSCRS, Baião, Gilbert, McEwan, & Carvalho, 2015) indicates that, whilst on average scoring lower on self-criticism than clinical-populations, non-clinical participants displayed a substantial variation in scores on this measure.

The element of MBCT focusing on developing a non-judgemental and accepting attitude of relating to experience may represent a novel or unfamiliar approach for those with a greater tendency towards self-criticism, who are prone to self-blame, a sense of defeat and feelings of inferiority (which inherently involve judging oneself), particularly when faced with unpleasant experiences such as negative emotions, stress or failure (Kannan & Levitt, 2013). Conversely, the focused-attention (or, concentration) element of MBCT could trigger the ‘driven-doing’ mode and further rumination and self-criticism (e.g. ‘why can’t I do this’, ‘I’m no good at anything I try’), when focused attention is inevitably not achieved and therefore a sense of defeat would be expected (as would be inevitable for novice meditators). In comparison, for those who are less prone to self-criticism (and by association, rumination and engaging in the ‘driven-doing’ mode), it is probable that adopting an accepting and non-judgemental attitude presents less of a novel approach and therefore less opportunity to develop, whereas cultivating focused-attention would be less problematic.

The standard MBCT MM guidance involves a greater emphasis on the acceptance of experience as it is, including mind wandering (Segal et al., 2012), and therefore what this could mean in practice is that MBCT courses for people with less of a tendency towards self-criticism

are likely to result in relatively reduced outcomes in comparison to those higher in self-criticism who would be predicted to benefit more. Put another way, trait self-criticism would be predicted to moderate the outcomes of MBCT, with people reporting higher levels of self-criticism showing more benefit than people reporting lower levels of self-criticism. In consideration of this hypothesised moderation effect, the question of whether MBCT can be tailored for people with a lower tendency towards self-criticism is raised. For these people, a greater emphasis on developing the present-moment focused, concentration component of MBCT may better facilitate engagement in the ‘being’ mode, and thus improved outcomes.

MBCT’s protocol defines an 8-week intervention period (Segal et al., 2012). However, brief mindfulness practices (10-15 minutes) have demonstrated increased levels of state mindfulness (Frieze et al., 2012; Hothrow et al., 2017; Slater et al., 2014; Weger et al., 2012), and improvements in state affect (Slater et al., 2014). The current study draws upon this experimental methodology to take the first step in the process of examining whether the efficacy of MBCT for people lower in self-criticism could be improved by modifying the form of MM guidance provided.

Compared to the outcome measure of wellbeing used in the Graham (2014) NHS staff MBCT study, which would not be expected to change over a brief timespan, state affect was selected as the primary outcome measure for the present study due to its sensitivity in detecting changes in mood following brief MM (e.g. Slater et al., 2014).

The following hypotheses were made:

1. A brief concentration-oriented MM will increase concentration significantly more than a brief acceptance-oriented MM. This would be expected based upon the explicit aim of such a meditation of increasing present-moment focus/concentration.

2. State mindfulness will significantly increase following both a 10-minute acceptance-oriented MM and a 10-minute concentration-oriented MM. This would be expected from the studies cited above using brief mindfulness meditation paradigms and because both forms of mindfulness meditation would be seen traditionally as increasing one's ability to cultivate mindfulness (Yates et al., 2017).
3. State affect (positive and negative) will significantly improve following both a 10-minute acceptance-oriented MM and a 10-minute concentration-oriented MM. This would be expected based upon the previously cited improvements in state affect following brief mindfulness meditations.
4. Trait self-criticism (high versus low) will moderate the effect of type of MM guidance (concentration versus accepting) on improvements in affect. Specifically:
 - Those high in trait self-criticism will demonstrate greater increases in positive affect following an acceptance-oriented meditation, compared to a concentration-oriented meditation.
 - Those low in trait self-criticism will demonstrate greater increases in positive affect following a concentration-oriented meditation, compared to an acceptance-oriented meditation.

The moderation effect was hypothesised for positive affect (PA) but not negative affect due to the literature demonstrating a strong correlation between self-criticism (as measured by the FSCRS) and low positive affect, but not between self-criticism and negative affect (Fritzsche, 2016).

Method

Design

A randomised within-participant experimental design was used, with all participants completing both an acceptance-oriented MM and a concentration-oriented MM in planned group sizes of between 8-12 people (although see discussion for issues arising around group size). The order in which participants completed MMs was counterbalanced to control for order effects. A within-participant design offered the benefit of increasing the power of the analysis obtained whilst allowing for a realistic recruitment target, compared to a between-participants design. Block randomisation of blocks of four was used, with the sequence generated using an online computer-generated randomiser (Sealed Envelope Ltd., 2019). The researcher was blind to each condition ordering defined by the allocation sequence until the beginning of each experimental session, after obtaining participant consent.

Participants completed outcome measures at baseline (T0), following the first meditation (T1), following the filler task (T2), and finally following the second meditation (T3). The independent variables (IVs) were: *meditation type* (concentration vs acceptance), *self-criticism* (low vs high) and *guidance order* (concentration first vs acceptance first). Dependent variables (DVs) were: state mindfulness, concentration, positive affect and negative affect.

Participants and Recruitment

Power calculations using G*Power (Faul, Erdfelder, Lang, & Buchner, 2007), indicated that for a four-way mixed measures ANOVA with post-hoc tests, a sample size of $n = 68$ would be required to attain a medium effect size ($f = .25$) with 80% power and $p < .05$.

A convenience sampling strategy was employed, with study advertisements (Appendix B) displayed around two university campuses, in addition to recruitment emails being sent to students at one site (Appendix C). A university student research portal system was also used to advertise the study at the second site (see Appendix D for ethical approval at this site), and participants recruited through this system were awarded research credits for their participation. Study advertisements also detailed the opportunity to win one of four £25 voucher prizes as an incentive to participate. Those under the age of 18 were excluded from the study and participating was discouraged if a person felt that taking part may be distressing for them. No other exclusion criteria were applied.

Those expressing an interest in the study were emailed a copy of the study information sheet to read (Appendix E) prior to signing up to an available group date, should they wish to continue. A total of 70 people participated, and their demographic information is detailed in Table 1. 25 (35.7%) participants reported never having meditated before, and of those remaining, 52 (64.2%) did not currently meditate, 7 (10%) meditated less than once a week, and 11 (15.7%) reported meditating more than once per week. Age of participants ranged from 18 to 38 years-old.

Ethical Approval

Ethical approval to conduct the study was granted by Canterbury Christ Church University (Appendix A), and British Psychological Society (BPS) Code of Human Research Ethics guidelines were adhered to at all times (BPS, 2014). Risks to participants and the researcher were deemed low, although due to reports of rare adverse experiences following MM in the literature (Van Dam et al., 2018), steps were taken to minimise this risk.

Table 1

Participant demographics, baseline characteristics and comparative statistical tests of baseline scores

Variable	Condition		Total	Group equivalence test statistic	<i>p</i>	<i>g</i>	
	Acceptanc e first (<i>n</i> = 37)	Concentrat ion first (n = 33)					
Gender: <i>n</i> (%)							
	Men	3	7	10 (14.3%)	$\chi^2(2, N = 70) = 3.21$.2	-
	Women	33	26	59 (84.3%)			
	Other	1	0	1 (1.4%)			
Ethnicity: <i>n</i> (%)							
	White	33	29	62 (88.6%)	$\chi^2(3, N = 70) = 3.24$.36	-
	Black	2	0	2 (2.9%)			
	Asian or Asian	2	3	5 (7.1%)			
	British Mixed race	0	1	1 (1.4%)			
Previous meditation experience							
	Yes	24	21	45 (64.3%)	$\chi^2(1, N = 70) = .01$.56	-
	No	13	12	25 (35.7%)			
Age (in years): <i>M</i> (<i>SD</i>)	21.3 (4.4)	21.6 (4.6)	$M = 21.4, SD = 4.5$	$t(68) = .23$.82	.06	
FSCRS self- criticism score: <i>M</i> (<i>SD</i>)	19.3 (8.33)	21.33 (6.4)	-	$t(68) = 1.14$.26	.03	
SMS score: <i>M</i> (<i>SD</i>)	57.95 (14.91)	54.61 (14)	-	$t(68) = .96$.34	.2	
Concentration score: <i>M</i> (<i>SD</i>)	80.92 (26.58)	68.36 (30.05)	-	$t(67) = 1.84$.07	.45	
PANAS PA score: <i>M</i> (<i>SD</i>)	26.11 (5.74)	25.45 (5.94)	-	$t(68) = .47$.64	.11	
PANAS NA score: <i>M</i> (<i>SD</i>)	14.16 (4.52)	15 (5.22)	-	$t(68) = .46$.64	.11	

Note: *g* = Hedge's *G* measure of effect size for unequal sample sizes (small = .2, medium = .5, large = .8); FSCRS = Forms of Self-Criticising/Attacking and Self-Reassuring Scale; SMS = State Mindfulness Scale; PANAS = Positive and Negative Affect Schedule; PA = Positive Affect; NA = Negative Affect.

These included the aforementioned discouragement of participation in study advertisement materials and participant information sheet, in addition to participants being offered a confidential space to discuss difficult experiences arising from their participation following

study completion and being given a written debrief detailing local sources of support (see procedure below for more details).

Materials

See Appendices F to I for copies of the measures detailed below.

State Mindfulness Scale (SMS). To assess whether the brief mindfulness practices influenced levels of state mindfulness, the SMS was employed. The SMS is a 21-item measure of state mindfulness which asks participants to rate aspects of their experience over a time period from 1 (“Not at all”), to 5 (“Very well”), with higher total scores indicating greater state mindfulness. The time period defined for this study was 10 minutes, reflecting the length of the mindfulness practices. Initial validation suggests the SMS is robust measure of a state of mindfulness (Tanay & Bernstein, 2013). In the present study, internal consistency of the SMS was high across all time points (T0-T3) and both conditions ($\alpha > .92$).

Forms of Self-Criticising/Attacking & Self-Reassuring Scale (FSCRS). The FSCRS was developed to measure both self-criticism and one’s ability to self-reassure (Gilbert et al., 2004), and consists of 22 items rated from 0 (“Not at all like me”) to 4 (“extremely like me”). The construct of self-criticism is composed of two forms: the *inadequate self* which focuses on personal inadequacy, with items such as “there is a part of me that puts me down”, and the *hated self* which measures self-persecutory and self-attacking beliefs such as “I call myself names”. The *hated self* construct is suggested to represent a more pathological aspect of self-criticism related to what would be typically classified as borderline personality difficulties (Gilbert et al., 2004), and has demonstrated floor effects in non-clinical samples (Gilbert et al., 2012). Therefore, for the purposes of this study employing a non-clinical population, only the subscale *inadequate self* was included as the measure of self-criticism.

The FSCRS has been found to be a robust and reliable measure of self-criticism using a normative sample (Fritzsche, 2016). In the current study, internal consistency of the *inadequate self* subscale was good at .89 (at T0).

Positive and Negative Affect Schedule (PANAS). The PANAS is a twenty-item scale designed to measure state affect (or mood), split along the dimensions of positive (PA) and negative affect (NA; Watson, Clark, & Tellegan, 1988). Ratings can indicate present moment experience, or over the past week and for this study, participants were directed to indicate their present moment experience. Participants rated their current experience from 1 (“Not at all”) to 5 (“Extremely”) on positively or negatively valenced words such as “interested”, “irritable” or “enthusiastic”. Higher PA, and lower NA scores indicate improved mood.

A large scale non-clinical sample (N = 1,003) study has demonstrated high reliability and validity of the PANAS (Crawford & Henry, 2004). Internal consistency in the current study was good for the ten PA and ten NA statements across all time points and conditions ($\alpha > .86$).

Concentration. A literature search uncovered very few paper-based self-report measures of concentration, with most studies employing neuropsychological forms of testing. The self-report measures that were found were poorly validated and lengthy in nature. Therefore, as a measure of concentration was not the primary outcome, but instead was intended primarily to be a manipulation check of whether the *concentration* meditation improved concentration as intended, a simple visual-analogue scale (VAS) of self-reported state concentration was designed, ranging from “I feel my concentration has been very poor” on the far left, to “I feel my concentration has been very good” on the far right. VASs have been demonstrated as a reliable and valid alternative to multi-item questionnaires in different contexts (e.g. de Boer et al., 2004; Hjerstad et al., 2011; Lee & Kieckhefer, 1989).

Mindfulness Meditations

The MM transcripts were based upon an MBCT mindfulness of breathing meditation (Segal et al., 2012) which invites participants to direct their focus towards noticing the present-moment experience of the breath, in a curious and accepting manner, as best they can. This form of MBCT meditation encourages an open acceptance of whatever arises in experience, with patience and kindness, and without judgement. This form of practice was therefore used for the *acceptance-oriented meditation* condition (see Appendix J for transcript).

For the *concentration-oriented meditation*, the same script was modified, thus increasing the valid comparability of both meditations. Modifications were made to shift the focus of the meditation towards greater concentration, for example:

“And throughout this sitting meditation practice the breath will always be present as something that you can return to...”

was changed to

“And throughout this sitting meditation practice the breath will always be present as something that you should do your best to concentrate on”;

and

“Knowing that, as it is the nature of all our minds to wander, if this is what we notice, this is fine. Noticing and experiencing the wandering mind is an important part of the practice and is a valuable opportunity to practise gentleness and patience, as best you can.”

was changed to:

“Knowing that it can be a challenge to sustain concentration and so if your mind wanders coming back to the breath, keeping in mind the intention here is to cultivate a concentrated state of mind”.

In addition, towards the beginning of the *concentration-oriented* meditation, participants were provided with instructions for certain techniques that could be deployed to further increase concentration during the practice. These were the lowering of the focal point of attention to a lower place in the body, such as the belly, if the meditator noticed a sense of restlessness or agitation; raising the focal point of attention higher in the body, if they experienced sleepiness; and applying curiosity to the changing experience of the breath, if they were feeling bored (see Appendix K for the *concentration* transcript).

As an additional way of reinforcing the subtle difference between practices, a brief explanation was given by the experimenter prior to listening to the second meditation, which highlighted how the next practice’s focus would differ from the first in terms of encouraging an acceptance of all experience (if the concentrated-orientated condition had occurred first) or a focus on cultivating focused concentration of the breath (if the acceptance-orientated condition had occurred first; see Appendices L and M for more details).

Both meditations were digitally recorded in a specialised quiet room, spoken by one of the study’s supervisors (FJ). Recordings were then edited to match the periods of silence within each and to be precisely 10-minutes long. Standardised instructions for how participants should position themselves prior to beginning a meditation were created and spoken aloud by the experimenter prior to the first meditation practice (Appendix N).

Filler task. A five-minute audiobook excerpt from the introduction to the book *A short history of nearly everything*, by Bill Bryson (2003), was chosen as a between-condition filler due to its relatively non-affective content and tone. The intended function of having

participants listen to this audio clip was to return state mindfulness, concentration and affect to baseline levels to try to minimise between-condition carry-over effects on these measures.

Procedure

Groups of up to 12 participants were tested in suitable rooms across both university campuses, with participants seated facing outward from a circular chair arrangement. This orientation was thought to reduce any non-verbal communication between participants. Following the experimenter briefing participants on the study procedure, participants were given the opportunity to ask any questions and then asked to sign consent forms. Once complete, the experimenter uncovered the randomised meditation ordering. Participants then provided demographic information and completed measures at T0 (PANAS, Concentration, the whole FSCRS, and SMS) using a pen and paper (see Figure 1 for a flow diagram of the experimental process).

Groups were then invited to follow the experimenter's instructions and modelling on how to adjust their posture in preparation for the first meditation practice, before following the first recorded practice for 10 minutes. After the meditation ended, participants were asked to complete the PANAS, concentration measure, and SMS (timepoint T1). The 5-minute filler was then played with participants who were then asked to complete the same measures again (T2), as the baseline for the next condition.

The participants were then informed of the different aims of the next meditation compared to the first, before being invited to return to their original posture and follow the second 10-minute meditation. Finally, participants completed the PANAS, concentration measure and SMS (T3), before being provided with a hard copy of a debrief to take home (Appendices O and P) and being given the chance to ask any questions. They were also

offered a space to discuss any difficult emotions or experiences that arose during the experiment individually with the researcher.

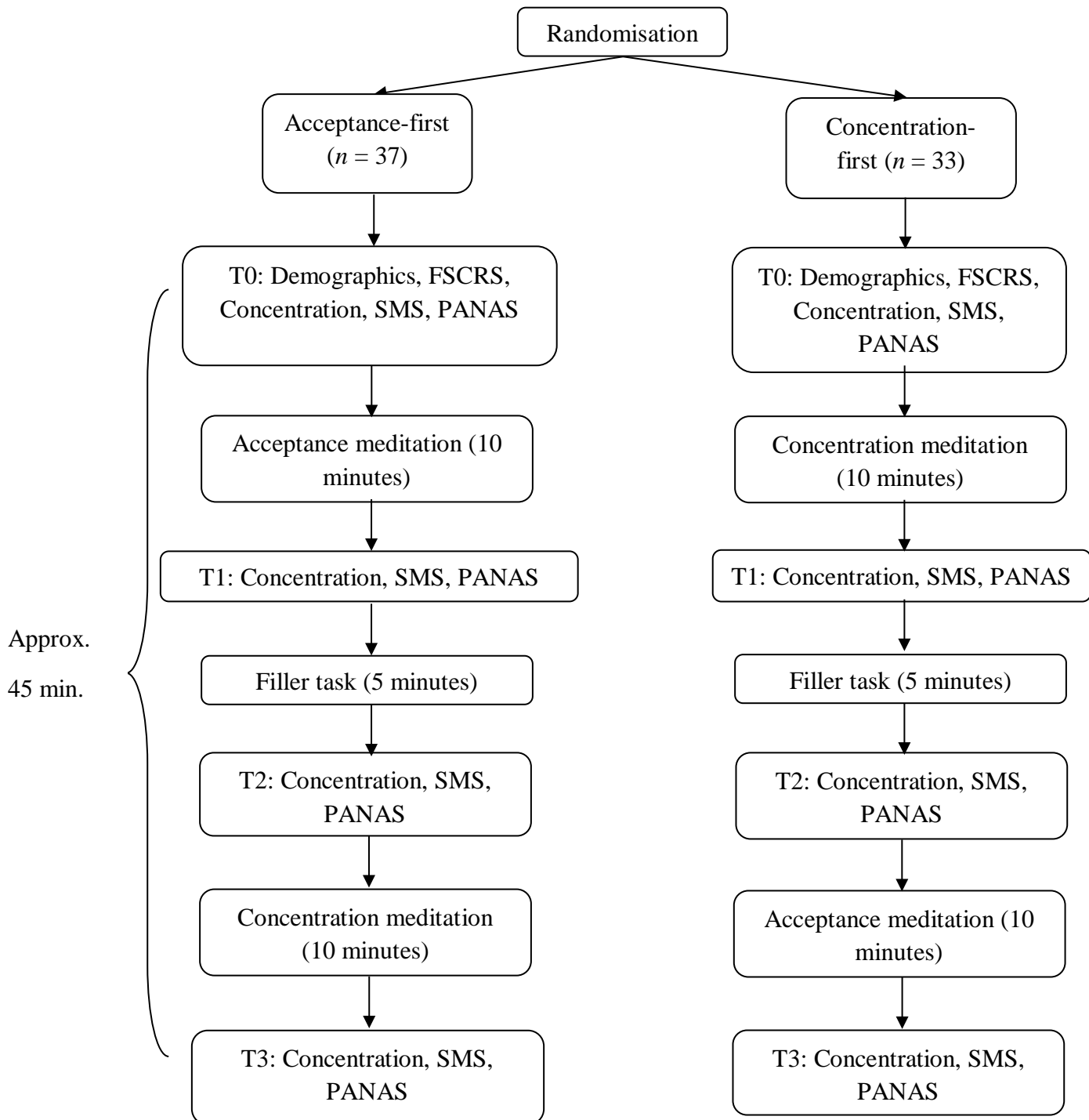


Figure 1. Experimental procedure flow diagram.

The debrief did not state the experiment's explicit hypotheses to prevent participants possibly sharing these with fellow students who were also potential future participants. However,

participants were offered the option of receiving a full explanation following study completion, along with a summary of the findings. The debrief also gave details of local resources (separate debriefs were created to reflect the different localities of the differing experiment locations), should participants experience any adverse effects from taking part which they did not feel able to discuss with the experimenter.

Data Analysis

SPSS v.23 was chosen for data analysis, and it was planned for the researcher to randomly select 20% of cases (14) entered into SPSS using an online number generator (Haahr, 2019), and to check these against the raw data to enhance confidence in input accuracy. It was decided to split scores on the IV *self-criticism* into high versus low self-criticism along the median value obtained, allowing for this categorical variable to be entered into a mixed ANOVA. Following initial exploration to check whether the data met parametric assumptions, four-way between (*guidance order x self-criticism*) x within (*time x meditation type*) mixed ANOVAs were planned for each of the DVs: state mindfulness, concentration, positive affect and negative affect. Here, a significant three-way interaction between *self-criticism*, *time* and *meditation type* would be indicative of a statistical moderation effect of self-criticism on the effects on the two types of mindfulness practice.

If *guidance order* was found to significantly interact with other IVs in this four-way ANOVA, this would suggest that there were carry-over effects between the conditions and hence invalidate the use of a within-participant design. In case of this eventuality, it was planned for only the first condition of each group to be entered into analysis using a three-way between (*meditation type x self-criticism*) x within (*time*) mixed ANOVA. Thus, the conditions would be compared in a between-participant design, avoiding the problem of the carry-over effects, but sacrificing power. If, on the other hand, *guidance order* was not found

to significantly interact with the other IVs then it would be collapsed across and the more powerful within-participants design used to test the hypotheses. In either case, it was planned to use post-hoc tests to explore significant interactions, with the application of the Bonferroni correction to control for family-wise alpha inflation due to multiple comparisons, where appropriate.

Results

Data Exploration and Checking

No errors were found following accuracy checking. One concentration score at T0 was missing and this case was excluded from the following analyses. No other data were missing. Data were then explored to check whether they met the assumptions of the planned parametric data analyses. Exploration identified outliers (data points greater than 2.5 SDs from the mean) on a number of measures and it was decided that all analyses would be run with these present and again with them deleted, to gauge their possible influence on the outcomes of statistical testing, with it being preferable to retain them due to them representing an observation that may be likely to generalise to the overall population. Ultimately, outlier removal did not result in changes to statistical testing outcomes and were therefore retained.

Standardised skewness and kurtosis tests, and Shapiro-Wilk p values suggested that multiple measures were non-normally distributed at different time points, although visual examination of P-P plots and histograms showed these to be relatively minor. In addition, Levene's test for homogeneity of variances suggested that multiple measures violated this assumption. However, the F -statistic has been demonstrated to be relatively robust against such violations (Donaldson, 1968; Lindman, 1974), and therefore analysis continued as planned.

Baseline Characteristics

Independent t-tests and chi-square tests were conducted on all baseline measures and demographic information, with no significant differences between participants in the *acceptance-first* and *concentration-first* conditions found on any variables (all $ps > .05$), confirming randomisation was successful. See Table 1 for mean baseline measure values and results from the above statistical tests. Self-criticism scores also varied widely as expected, from 1-34 (higher indicates greater self-criticism).

Table 2

Interactive effects of guidance order across outcomes

		<i>F</i>	<i>p</i>	η^2_{partial}
Guidance order x meditation				
	Mindfulness	2.21	.032*	.07
	Concentration	9.89	.002*	.13
	Positive affect	11.16	.001*	.15
	Negative affect	10.96	.002*	.15
Guidance order x meditation x time				
	Mindfulness	.96	.33	.02
	Concentration	5.58	.02*	.08
	Positive affect	7.56	.008*	.1
	Negative affect	8.51	.005*	.12
Guidance order x meditation x time x self-criticism				
	Mindfulness	.002	.97	<.001
	Concentration	1.16	.29	.02
	Positive affect	.36	.55	.01
	Negative affect	1.62	.21	.03

Note. η^2_{partial} = partial eta squared effect size (small = .01, medium = .05, large = .14); * = significant effect.

Hypothesis Testing

To determine whether the order in which participants completed each form of meditation practice impacted on outcomes, the four-way mixed ANOVA planned above was run. Unfortunately, *guidance order* significantly interacted with *meditation type* across all DVs (all $ps < .05$), indicating that the order in which participants completed each meditation

affected outcomes (see Table 2 for details of the analyses). Therefore, given that carry-over effects had been found, invalidating a within-participant design, only the data for the first meditation that participants completed were analysed (as described above in the data analysis section of the method) resulting in a between-participants design for the subsequent analyses.

Revised Hypothesis Testing

Given the order effects reported above, three-way mixed ANOVAs (*time x meditation type x self-criticism*) were run for each of the DVs. Table 3 displays the mean DV values and standard deviations across time points and condition (see Appendix Q for a table breaking these down across levels of self-criticism), and Table 4 displays the *F* statistics, *p* values and partial eta squared effect size estimates of the main effects and interactions from these ANOVAs.

Hypothesis 1: A brief concentration-oriented MM will increase concentration significantly more than a brief acceptance-oriented MM.

A main effect of time was found on the self-reported measure of state concentration, indicating that across both meditation types, participants reported their level of concentration to have increased significantly. The significant *time x meditation type* interaction suggested that changes from baseline to post-meditation concentration were differentially impacted by the form of meditation.

Follow-up tests showed that concentration scores significantly increased in both the *concentration* condition ($t(32) = 7.04, p < .001, d = 1.23$) and the *acceptance* condition ($t(35) = 2.32, p = .026, d = .39$), with a significantly greater increase in perceived concentration in the *concentration* condition compared to the *acceptance* condition ($F(1,67) = 7.55, p = .001, \eta^2_{\text{partial}} = 1.01$). This confirmed the hypothesis that a concentration-oriented meditation would

increase perceived levels of concentration significantly more than an acceptance-oriented meditation (see Figure 2 illustrating this interaction).

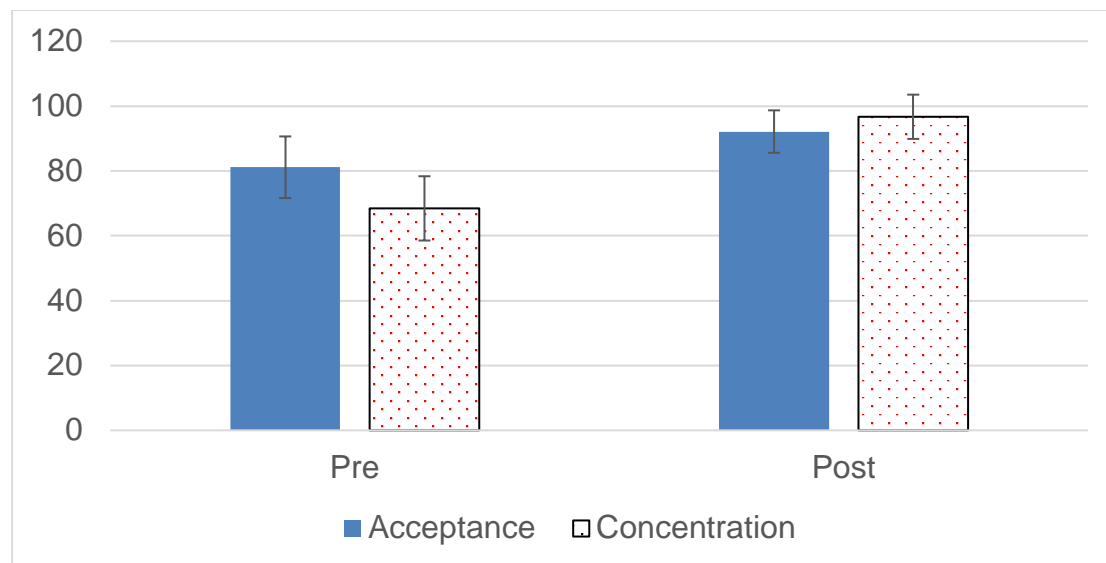


Figure 2. Pre- and post-meditation mean concentration scores organised by meditation type.

Hypothesis 2: State mindfulness will significantly increase following both a 10-minute acceptance-oriented MM and a 10-minute concentration-oriented MM.

As can be seen in Table 4, a significant main effect of time was found for state mindfulness, suggesting that state mindfulness significantly increased across both meditation types. The non-significant time x meditation type interaction suggested that the increases in state mindfulness did not significantly differ between the two MMs.

Hypothesis 3: State affect (positive and negative) will significantly improve following both a 10-minute acceptance-oriented MM and a 10-minute concentration-oriented MM.

Analyses found no main effect of *time* on PA, indicating that neither form of meditation was associated with significant increases in PA. Therefore, this part of the hypothesis was not confirmed. However, analyses indicated a main effect of *time* on NA, showing that both meditation types were associated with significantly reduced NA from baseline levels, therefore confirming this part of the hypothesis. Further, the absence of a

significant *time x meditation* type interaction shows no between-group differences in pre-post meditation reductions in NA.

Table 3

Means and standard deviations of outcome variables organised by time and condition

	Acceptance condition		Concentration condition	
	Pre- mean (SD)	Post- mean (SD)	Pre- mean (SD)	Post- mean (SD)
Mindfulness	57.95 (14.91)	74.81 (12.86)	54.61 (14)	78.12 (11.56)
Concentration	80.92 (26.58)	91.97 (22.45)	68.36 (30.05)	96.73 (15.53)
Positive affect	26.11 (5.74)	26.38 (7.26)	25.45 (5.94)	24.39 (7.5)
Negative affect	14.46 (4.52)	12.62 (4.54)	15 (5.22)	12.82 (3.76)

While not directly testing this hypothesis, it is also worth noting that a significant *self-criticism x time interaction* was found and examination of means indicated that for those high in self-criticism, both forms of meditation were associated with a lowered NA from baseline levels compared to those low in self-criticism. Put in another way, a non-specific, statistically significant moderation effect of self-criticism on NA was found for both meditation types (see Figure 3 for a visualisation of this interaction). Further analysis revealed that baseline levels of NA were significantly higher in the high self-critical participants than the low ($t(68) = 4.69, p < .001, d = 1.12$). Furthermore, post-hoc dependent-means t-tests showed that those low on self-criticism showed no pre-post change in NA across meditations ($t(34) = .04, p = .97, d = 0.02$), whereas a significant improvement in NA was shown for those high on self-criticism ($t(34) = 4.76, p < .001, d = 0.81$).

Hypothesis 4: Trait self-criticism (high versus low) will moderate the effect of type of MM guidance (concentration versus accepting) on improvements in affect.

Table 4

Main effects and interactions with each dependent variable

		<i>F</i>	<i>p</i>	η^2_{partial}
Time				
	Mindfulness	118.81 (1, 66)	<.001*	.64
	Concentration	37.97 (1, 65)	<.001*	.37
	Positive affect	.35 (1, 66)	.55	.05
	Negative affect	12.83 (1, 66)	.001*	.16
Time x meditation				
	Mindfulness	3.19 (1, 66)	.078	.05
	Concentration	7.34 (1, 65)	.009*	.1
	Positive affect	.87 (1, 66)	.35	.01
	Negative affect	.04 (1, 66)	.84	.00
Time x self-criticism				
	Mindfulness	.09 (1, 66)	.076	.00
	Concentration	.15 (1, 65)	.072	.00
	Positive affect	.17 (1, 66)	.68	.00
	Negative affect	12.37 (1, 66)	.001*	.16
Time x meditation x self-criticism				
	Mindfulness	.34 (1, 66)	.56	.01
	Concentration	.51 (1, 63)	.48	.01
	Positive affect	2.57 (1, 66)	.11	.04
	Negative affect	.002 (1, 66)	.96	.00

Note. η^2_{partial} = partial eta squared effect size (small = .01, medium = .05, large = .14); * = significant effect

Sub-hypothesis 1. Those high in trait self-criticism will demonstrate greater increases in positive affect following an acceptance-oriented meditation, compared to a concentration-oriented meditation.

A significant three-way *self-criticism x time x meditation type* interaction was not found on the PA outcome, meaning that no moderation effect of self-criticism was found and that the null hypothesis should be retained. This is perhaps unsurprising given the absence of a main effect of *time* on PA.

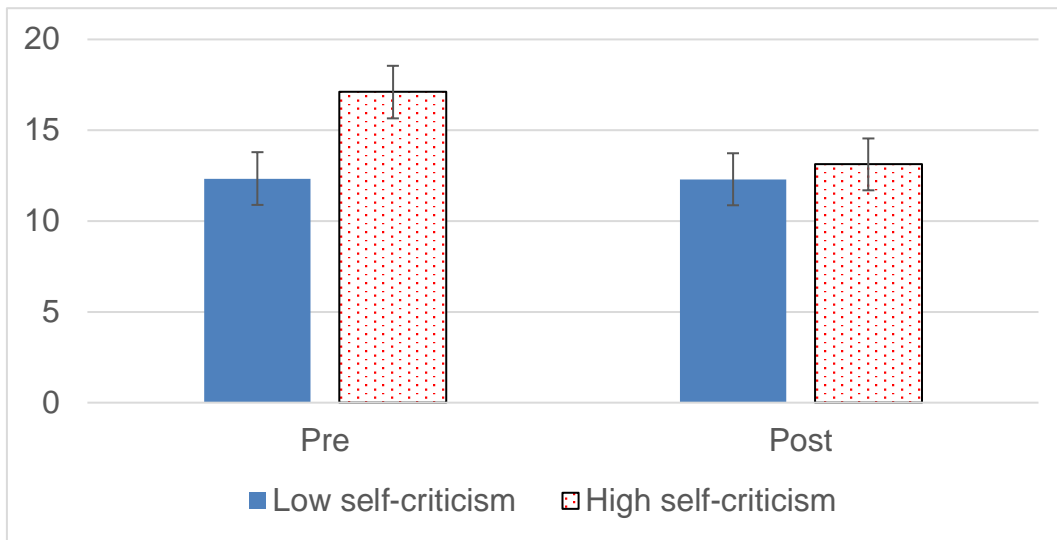


Figure 3. Mean pre- and post-meditation NA scores organised by self-criticism.

Sub-hypothesis 2: Those low in trait self-criticism will demonstrate greater increases in positive affect following a concentration-oriented meditation, compared to an acceptance-oriented meditation.

The lack of a significant three-way *self-criticism x time x meditation type* interaction on PA as described above means that this hypothesis was also not confirmed.

Discussion

This study aimed to determine whether trait self-criticism would interact with the type of instruction given during a brief, 10-minute MM (*acceptance-oriented* versus *concentration-oriented*) to moderate the benefits of these meditations on positive affect. The findings will now be discussed in relation to the research hypotheses with consideration of the wider literature, before the limitations of the research, clinical implications and future research directions are considered.

Effects of Meditation Type on Perceived Concentration

The hypothesis that the *concentration-oriented meditation* would increase participant ratings of concentration to a greater degree than the *acceptance-oriented meditation* was

confirmed. Further, analysis indicated that concentration was significantly improved following both meditations.

The increase in concentration for the *acceptance* condition would be expected when considering that an attentional focus on the breath is still a focus of this MM and the original MBCT MM instructions, albeit to a lesser extent than applying a non-judgemental and accepting attitude to experience. Evidence from event-related potential (ERP) studies has shown that concentration does indeed increase following MBCT for depression (Bostanov, Keune, Kotchoubey, & Hautzinger, 2012; Bostanov, Ohlrogge, Britz, Hautzinger, & Kotchoubey, 2018). Further, it appears that by modifying the MBCT instructions to place a greater emphasis on concentration, as in the *concentration* condition, this effect is boosted, as perceived concentration was greater in the *concentration* than in the *acceptance* arm. However, whilst participants were not told what the focus of the first meditation was before completing it, it is possible that the greater increase of concentration in the *concentration* condition demonstrates a demand effect where participants were aware from the meditation instructions that they should try their best to concentrate on the breath. Further, the concentration measure used was unvalidated and its reliability is unknown, meaning that the observed findings must be interpreted with caution and as preliminary only.

Effects of MM on State Mindfulness

Based upon previous experimental research (e.g. Frieze et al., 2012), it was hypothesised that a brief 10-minute MM would significantly increase state mindfulness. A significant main effect of *time* on the SMS confirmed this prediction, showing state mindfulness increased following both meditations. No prediction was made regarding a difference between the two types of meditation, and there was a non-significant *time x meditation* interaction, although the study was not designed to test equivalence. Therefore,

this study adds to the existing literature demonstrating that a brief MM is associated with improvements in state mindfulness and suggests that this may be true for both concentration and acceptance instructions.

Effects of MM on State Affect

The hypothesis that both forms of meditation would significantly improve affect was partially confirmed, where PA failed to change from baseline levels, but NA was significantly reduced following each meditation. A non-significant *time x meditation* interaction confirmed that neither meditation was more effective in reducing NA. In addition, a significant interaction between *time* and *self-criticism* was found, and follow-up post-hoc tests showed that those scoring highly on the *inadequate-self* subscale of the FSCRS saw a significant reduction in NA following the meditations, compared to no reduction for those scoring low on *inadequate-self*.

The significant reduction across conditions, and therefore improvement on the NA subscale of the PANAS is in line with existing research utilising brief MMs (Frieze et al., 2012; Hopthrow et al., 2017; Slater et al., 2014; Weger et al., 2012), and also with 8-week MBCT (Collard, Avny, & Boniwell, 2008; Schroevers & Brandsma, 2010) and MBSR programmes (Shapiro, Brown, & Biegel, 2007). This reduction in NA following both brief and full mindfulness-based intervention protocols adds confidence in the utility of the brief meditation experimental paradigm to being used as a form of ‘testbed’ where the effects of modifications to meditations could be tested more quickly than a full intervention, and in a highly-controlled way.

Regarding the finding that those scoring highly on *self-criticism* at baseline demonstrated a significant reduction in NA following each meditation compared to no observed reduction for those scoring low on *self-criticism*, this may indicate that only those

more prone to self-criticism benefit from a brief MM practice (irrespective of which form) when compared to those low on self-criticism. Concentrating on the breath might help to bring attention away from self-critical thoughts (as in the *concentration-oriented* meditation), whilst being encouraged to adopt a non-judging and accepting attitude towards experience (*acceptance-oriented* meditation), might enable people to be more accepting of self-critical thoughts. Where self-criticism might be leading to the relatively higher NA observed at baseline, the MMs might then directly target this cause of higher NA through the mechanisms proposed above. In contrast, the cause(s) of the NA present for those low on self-criticism at baseline may be different (i.e. not getting caught-up in the ‘driven-doing’ mode as a result of self-critical thinking), and therefore the lack of improvement on NA in this group might indicate that this cause(s) is less amenable to change through MM. Further research could explore this further to confirm whether those low in self-criticism are indeed unlikely to benefit from either an *acceptance-oriented* or *concentration-oriented* MM.

Furthermore, this effect of each MM on NA does not support the research validating the FSCRS which concluded that PA, but not NA significantly correlated with self-criticism (Fritzsche, 2016). However, other research does suggest that high self-criticism is predictive of NA; for example Dunkley, Zuroff and Blankstein (2003) showed self-criticism to be associated with high daily negative affect and low daily positive affect over extended periods (one week or more), indicating a relatively stable high level of NA in those more prone to self-criticism. Research by Zuroff et al. (2016) tracking college students on various outcomes over seven days, including self-criticism and affect, found a similar pattern of results.

One possible explanation for this discrepancy in findings is how Fritzsche (2016) elected to analyse the FSCRS: by calculating a total score. This mixing of the three FSCRS subscales may have masked the positive association between the *inadequate-self* subscale and NA seen in the present study, as the *reassured-self* would be expected to negatively correlate

with NA. Factor analysis of the FSCRS has strongly suggested a three-factor structure, and hence three-subscale format of the scale (Baião et al., 2015), and these authors do not recommend using a total FSCRS score as a measure of self-criticism.

The lack of improvement on PA across time was unexpected, considering that previous research utilising brief MMs had demonstrated an experimental effect (e.g. Frieze et al., 2012; Slater et al., 2014). It is possible that this null effect represents a Type II error resulting from the study's reduction in power following the decision to analyse the first meditation outcomes only. However, this seems unlikely as examination of pre- and post-meditation mean PA scores show small changes of less than 1.5 across time. Moreover, further consultation of the literature did uncover two studies which also produced a null result for PA: Tsai et al. (2017) in the context of applying a 10-minute MM to undergraduate females following a food consumption task, saw no change in PA; and Collard et al. (2008) investigating the impact of a full 8-week MBCT programme. In addition, Westbrook (2013) in an unpublished experimental mindfulness induction task demonstrated a paradoxical decrease in PA. Therefore, it appears that the ability of a brief MM to improve PA remains unclear and further research is required.

One explanation offered by authors (Collard et al., 2008; Westbrook, 2013) for the unexpected changes (or lack thereof) they observed on the PA subscale of the PANAS is that this subscale is biased towards terms related to activity and energy such as “excited” and “active”, which therefore neglects possible other dimensions of positive affect such as tranquillity, serenity, contentment and calm which would be more expected to follow from an activity which involves sitting quietly in stillness. Research by Gilbert et al. (2008) has recently suggested that positive affect is in fact comprised of three separate forms of positive affect: activated positive affect, relaxed positive affect and safe/content positive affect. The self-report scale used in Gilbert et al.'s research may therefore be a more valid and inclusive

measure of positive affect than that contained within the PANAS which was originally developed in 1988 (Watson et al., 1988), and would be expected to have greater sensitivity to detect the changes in positive affect predicted in this study. Future studies could utilise both scales to test this assertion.

Positive Affect and Self-Criticism

It was hypothesised that participants with relatively higher self-criticism would demonstrate greater increases in PA following an *acceptance-oriented meditation*, compared to a *concentration-oriented meditation*, and that participants scoring low on self-criticism would demonstrate greater increases in PA following a *concentration-oriented meditation*, compared to an *acceptance-oriented meditation*. However, the lack of significant three-way interaction of *self-criticism* with *time* or *meditation type* meant that this hypothesis was rejected. *Self-criticism* was not found to moderate effects on PA across either meditation type. Considering the lack of change on PA discussed above, it is not surprising that a moderating effect of *self-criticism* on this outcome was absent. It is possible therefore that the use of the three-factor positive affect scale identified above (Gilbert et al., 2008) would have uncovered significant improvements in PA following the meditations which in turn might have allowed for the hypothesised moderation effect to emerge. However, the reduced power of the experiment, and therefore possibility of a Type II error, would likely have precluded the demonstration of a significant moderation effect despite a more sensitive and valid measure of the PA outcome.

Alternatively, it may be that even with adequate experimental power and a superior PA measure, the hypothesised moderation effect of *self-criticism* on PA would not be found, and further research is required to clarify this.

Clinical Implications

This study's null finding of a moderating effect of self-criticism and type of meditation on mood is consistent with the possibility that MBCT's acceptance-oriented style of meditation of the breath is helpful in improving negative affect in those with high self-criticism in a non-clinical population. However, the lack of improvement on negative affect seen for those low on self-criticism raises doubts as to whether MBCT's acceptance-oriented MM style is of benefit to these people, and this also applies to concentration-oriented MMs. Future research, particularly with more diverse non-clinical populations, is required in order to replicate this before any recommendation is made to amend which non-clinical populations are offered MBCT.

Further, the findings indicate that people scoring higher on self-criticism don't benefit less from concentration versus acceptance practices and therefore that both could be offered, dependent on the intention of the practice to either cultivate concentration, or a non-judgemental and accepting attitude. However, this conclusion cannot be generalised to people from clinical populations with clinically high levels of self-criticism (such as those with a diagnosis of depression). It could be that moderating effects of self-criticism on the outcomes of differing MMs would be found in such a population and this possibility warrants careful investigation due to the possible negative impact of concentration instructions through the originally hypothesised triggering of the 'driven-doing' mode following inevitable failure to maintain concentration.

Limitations

In addition to the lack of experimental power (and therefore inflated possibility of Type II errors occurring) for finding the hypothesised moderating effect of *self-criticism* on PA, the measure of concentration used was not validated and future research would benefit from

establishing convergent validity of this measure with reliable neuropsychological measures such as the Sustained Attention to Response Task (SART, Silverstein, Light, & Palumbo, 1998), in addition to assessing its test-retest reliability.

Regarding the former point, this resulted from a failure of the filler task to return participant state mindfulness, state affect and state concentration to baseline levels as intended. A change of the analysis was then necessitated, comparing only the outcomes of the first meditation completed by participants, therefore precluding the intended use of the within-participant design of participants acting as their own controls. Further, the lack of a separate control condition means that the main effects of time demonstrated cannot be firmly attributed to the fact participants meditated and preclude causal interpretations where both meditation conditions improved with non-significant between-group differences. In hindsight, running a small pilot study to test the experimental design would have been valuable in potentially avoiding this limitation. However, as will be discussed below, it remains likely that any form of within-participant design may be poorly suited to a brief meditation experimental approach because of potential carry-over effects, and therefore a between-participant design would have ultimately been used anyway. This would then have necessitated the use of a separate control condition in order to have confidence that any effect found was attributable to the experimental manipulation (*meditation type*), and the number of participants required to attain 80% power in this design was 120. It remains likely that a recruitment target this high would have been beyond the scope of this study.

One way that the filler could have been altered to increase its likelihood of success would be to increase its length in order to maximise the hypothesised boredom effect, therefore raising NA. However, no evidence could be found regarding how long the experimentally induced changes in affect following brief MMs endure, and it is possible that they in fact endure far beyond the scope of a single experimental session (for example,

hours). Whilst a negative mood induction task such as asking participants to memorise a list of negatively-valenced words (e.g. Malhi et al., 2007) would be hypothesised to counter the carry-over effect of reduced NA which was observed, this would also likely lower PA levels. Therefore, elimination of carry-over experimental effects within the context of a repeated measures, pre-post experimental design as used in this study may not be a viable one in the context of MM.

A second limitation resulted from the group-based nature of the study, where the size of each experimental group differed across sessions. This was due to factors such as participants not arriving at sessions as planned, and the time-limited nature of the research meaning that it was not possible to await group numbers filling to the maximum of twelve before running a session. Despite the present study not including any group discussion, this variation may have introduced differences on outcomes due to the relative presence or absence of non-specific group effects. However, one pilot study comparing group-based MBCT and individual MBCT for people diagnosed with a somatic illness and comorbid depressive symptoms found no differences on measures of depressive symptoms, anxiety, well-being, mindfulness and self-compassion (Schroevers, Tovote, Snippe, & Fleer, 2016). Whilst this suggests that MBCT outcomes are not influenced by group effects, it remains possible that in different populations, including non-clinical such as in the present study, non-specific group effects may work to affect outcomes; for example, completing potentially quite sensitive measures, such as the FSCRS may have increased feelings of self-consciousness which might be heightened when surrounded by others. Further research comparing individual ultra-short with group-based mindfulness experiments, would help to clarify this.

Finally, the relatively homogeneous sample of white females in higher education with an average age of 21.4 years means that the current findings' generalisability is limited. Future studies would benefit from including a more diverse sample.

Future Research

To build on the current study which to this author's knowledge is the first to investigate whether participant characteristics have the potential to moderate the outcomes of a brief MM, future research would benefit from further investigating the hypothesised role of trait self-criticism on affect to provide a more definitive answer than that currently presented. This research could employ a sample large enough to detect small to medium effect sizes; use a pre-post between-participants design and include a control group to avoid the possible contamination of results by carry-over effects as seen in the current within-participant design; design, validate and use a brief measure of state concentration to improve confidence in this outcome; and replace or complement the PA subscale of the PANAS with the three-factor PA scale developed by Gilbert et al. (2008).

In addition, the investigation for the presence of other potential moderators, whether using a brief meditation paradigm or full MBCT course may prove clinically useful for clinical and non-clinical populations, as this could allow for modifications to meditation instruction in a similar way as the present study, to accommodate for moderating effects and therefore improve outcomes for groups of people who may stand to benefit less from MBCT in its current form. Gender is one such potential moderator in a non-clinical population which has recently been identified in a 12-week university-based mindfulness-based intervention (MBI), where women experienced greater decreases in NA than men (Rojiani, Santoyo, Rahrig, Roth, & Britton, 2017). The author therefore suggest that men may require a gender-

specific form of MBI which is modified to address the particular forms of coping that they tend to use compared to women.

Conclusions

The primary focus of this study was to investigate whether the degree a person evidences *self-criticism* - a characteristic behaviour which is highly predictive of rumination, which is in turn highly predictive of developing depression – interacts with the type of MM instruction given (acceptance-oriented vs concentration-oriented) to moderate state positive affect, a commonly investigated outcome of brief mindfulness practices. No evidence was found to support this hypothesis, and PA was not found to change in response to either an *acceptance-oriented meditation*, or a *concentration-oriented meditation* which was unexpected. The study did demonstrate that NA was significantly reduced, regardless of which form of meditation participants completed, but only for those scoring highly on *self-criticism*, which questions the efficacy of brief MMs in improving mood for those scoring low on *self-criticism* (and by extension, the efficacy of MBCT). Further, as hypothesised, the *concentration-oriented* meditation resulted in a significantly greater increase on a self-reported state concentration measure when compared to the *acceptance-oriented meditation*, although this finding should be treated as preliminary due to the novel nature of the measure of concentration. Finally, both meditations were associated with significant increases in state mindfulness with no differences between conditions.

These results provide preliminary evidence for the possibility of modifying MBCT instructions to achieve a difference in desired effect (i.e. fostering the skill of concentration), and they further replicate the potential of the brief MM paradigm to be associated with an increase in state mindfulness, and a decrease in NA. Given the continued widening application of MBCT to groups other than it was originally developed for, it is important for

future research to determine whether there are factors present in these groups which may influence the extent to which people can benefit from MBCT, and in turn, whether these can be accommodated for.

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Appendix A

Ethical approval letter

(This has been removed from the electronic copy)

Appendix B

Study advertisement poster

The poster is divided into three main sections. The top section has an orange background with the title 'EXPERIENCE THE BENEFITS OF MINDFULNESS' in large, blue, hand-drawn letters, followed by 'AND HELP OTHERS TOO!' in smaller, black, hand-drawn letters. The bottom-left section features a blue background with a photograph of a woman in a meditative pose, surrounded by water splashes. Text in this section includes 'For those who have little or no experience of mindfulness...' and '...or the curious, experienced meditator...' in white, and 'WE'D LIKE YOUR HELP IN ADDING TO THE EVIDENCE-BASED AND IMPROVING THE BENEFITS PEOPLE OFTEN EXPERIENCE.....' in white. A list of benefits is shown in yellow: 'IMPROVED: WELL-BEING, MOOD, FOCUS, RELATIONSHIP SATISFACTION'. The bottom-right section has a yellow background with black text stating 'WE WILL BE RUNNING 1 HOUR GROUPS AT A CONVENIENT TIME AND PLACE.' and 'THERE'S A CHANCE TO WIN ONE OF 4 £25 PRIZES!'. It also says 'FOR MORE INFORMATION OR TO SIGN UP' and 'CONTACT' above a blue rectangular box.

**EXPERIENCE
THE BENEFITS
OF MINDFULNESS**

AND HELP OTHERS TOO!

**For those who have little or no
experience of mindfulness...**

**...or the curious,
experienced meditator...**

**WE'D LIKE YOUR HELP IN ADDING TO THE EVIDENCE-BASED
AND IMPROVING THE BENEFITS PEOPLE OFTEN
EXPERIENCE.....**

IMPROVED:

**WELL-BEING
MOOD
FOCUS
RELATIONSHIP
SATISFACTION**

**WE WILL BE
RUNNING 1 HOUR
GROUPS AT A CONVENIENT
TIME AND PLACE.**

**THERE'S A CHANCE TO
WIN ONE OF 4 £25
PRIZES!**

**FOR MORE
INFORMATION
OR TO SIGN UP**

CONTACT

Appendix C

Recruitment email

Dear student,

As part of my Doctorate in Clinical Psychology at the Salomons Centre for Applied Psychology (Canterbury Christ Church University) I am conducting a study investigating the effects of meditation practice on people's mood. The study aims to further contribute to the existing literature demonstrating the positive effects of mindfulness meditation on mood in addition to investigating how these effects might be further improved.

What does taking part involve?

Participants will be asked to attend a one-hour, one-off group session with up to 11 other participants. This session would involve completing a number of measures, including demographic information, and listening to two guided mindfulness meditation recordings. All of the data collected will be used solely for research purposes and stored securely, in accordance with university policy. Canterbury Christ Church University has given full permission for this study to be conducted.

Interested?

If you are interested in taking part or would like further information, please contact XXXX at [XXXX](#)

Groups will be held on campus or at a convenient nearby facility. Following initial contact and an expression of interest, a poll with potential group dates and times will be emailed to you to complete. Group dates will then be distributed.

All the best,

XXXXXX

Trainee Clinical Psychologist

Salomons

Canterbury Christchurch University

Appendix D

Second site ethical approval

(This has been removed from the electronic copy)

Appendix E

Participant information sheet

Information about the research

Adapting mindfulness to the individual

Hello. My name is XXX and I am a trainee clinical psychologist at Canterbury Christ Church University. I would like to invite you to take part in a research study. Before you decide it is important that you understand why the research is being done and what it would involve for you.

Talk to others about the study if you wish.

(Part 1 tells you the purpose of this study and what will happen to you if you take part. Part 2 gives you more detailed information about the conduct of the study).

What is the purpose of the study?

There now exists a wealth of evidence to suggest that for non-clinical populations, mindfulness meditation can improve things such as wellbeing, mood, focus and relationship satisfaction, to name but a few. This study is aiming to add to this evidence-base by further investigating the effects of mindfulness meditation and ways that its effects might be improved.

Why have I been invited?

You have been invited because you are a student of either XXXX or XXXX. I hope to recruit a total of 68 people to take part in the study.

Do I have to take part?

Your participation in this study is entirely voluntary. If you agree to take part, I will then ask you to sign a consent form. You are free to withdraw at any time, without giving a reason.

What will happen to me if I take part?

You will be asked to spend one hour alongside a group of around 9 other participants, completing a number of psychometric measures before, in-between and after engaging with two digital pre-recorded guided mindfulness meditation practices. The practices will be separated by a short task. To finish, a short debriefing of the study will be provided and the opportunity to ask any questions. The session will likely be conducted in a suitable room on campus.

Expenses and payments

As a thank you for taking part, you will be given the opportunity to be entered into a prize draw with a chance to win one of four £25 prizes. Your email address will be required if you would like to be entered into the draw.

What are the possible disadvantages and risks of taking part?

Whilst most people find mindfulness meditation to be a positive and enjoyable experience, very occasionally, some people may become distressed by taking part in a mindfulness meditation, and we would recommend that if you are currently experiencing mental health difficulties or suspect that participating may be a distressing experience, you do not take part in the study.

What are the possible benefits of taking part?

We cannot promise that the study will help you due to its brief, experimental nature. However, it is hoped that the information gathered from the study will help to improve the outcomes for people taking part in mindfulness-based interventions in the future. It is also quite possible that short-term positive effects on mood will be experienced and this may encourage you to pursue mindfulness meditation independently.

What if there is a problem?

Any complaint about the way you have been dealt with during the study or any possible harm you might suffer will be addressed can be raised with the lead researcher at any time during the study session. Alternatively, you will be able to contact the Salomons Research Director to voice any concerns or make a complaint (see Part 2 below).

Will information from or about me from taking part in the study be kept confidential?

Yes. We will follow ethical and legal practice and all information about you will be handled in confidence. The details are included in Part 2.

This completes part 1.

If the information in Part 1 has interested you and you are considering participation, please read the additional information in Part 2 before making any decision.

Part 2

What will happen if I don't want to carry on with the study?

You can withdraw from the study at any time up until its submission as part of a Major Research Project contributing towards the lead investigator's doctoral degree. If you choose to withdraw before this point, any data collected from you will be destroyed/deleted and removed from any analyses used in the project.

What if there is a problem?

Any complaints made by you will be taken very seriously and reviewed by the lead investigator, and if necessary, Canterbury Christ Church University.

Complaints

If you have a concern about any aspect of this study, you should ask to speak to the lead researcher who will do their best to address your concerns. Contact via email can be made to: XXXX. If you remain unhappy and wish to complain formally, you can do this by contacting Professor Paul Camic, Research Director, Salomons Centre for Applied Psychology – paul.camic@canterbury.ac.uk, tel: 03330 117114

Will information from or about me from taking part in the study be kept confidential?

Every measure will be taken to ensure that the data you provide during your participation in the study will be kept safe, secure and strictly confidential. The information below explains how this will be done:

- The data you provide will be on paper hard copies.
- An anonymous coding system will be used so that only lead researcher will have the ability to link your personal information to the data that you provide as part of the study. The temporary paper document linking unique participant codes will be stored in a locked, secure filing cabinet and once transferred to a password-protected document, the paper document will be destroyed.
- The data collected will be reported anonymously in the lead researcher's Major Research Project and also submitted for publication in the journal *Mindfulness*. Your data would be kept securely at the Salomons Centre for Applied Psychology for 5 years, after which time it will be destroyed. The researchers may wish to use the data you provide in subsequent publications.
- The lead researcher will be the only person who has access to any personally identifiable information.
- In the event that the lead researcher has any concerns to your safety or the safety of others, confidentiality may no longer be able to be maintained.
- You have the right to check the accuracy of data held about you and correct any errors.

Who is organising and funding the research?

This study was organised by the lead researcher XXX with input from a lead supervisor (XXXX) and external supervisor XXXX, as part of the lead researcher's Clinical Psychology doctoral training at Canterbury Christ Church University. The university has funded this study.

Who has reviewed the study?

This study has been reviewed and given favourable opinion by The Salomons Ethics Panel, Salomons Centre for Applied Psychology, Canterbury Christ Church University and the XXXX Ethics Panel.

Should you wish to take part, you will be given a copy of this information sheet to keep for your records in addition to a copy of your signed consent form.

Requesting study results

If you would like a copy of the research study following its completion, please email me at: XXXX and I will be happy to send you a copy.

Further information and contact details

Should you require further information regarding this research project, please do not hesitate to contact me via email at: XXXX. Alternatively, you can leave a message for me on a 24-hour voicemail phone line at 03330 117070. Please say that the message is for Luke Slater and leave a contact number so that I can get back to you.

If you would like to speak to the member of the research department who is supervising the study and is very experienced in this field of research, please contact XXXX

Appendix F

State Mindfulness Scale

(This has been removed from the electronic copy)

Appendix G

Forms of Self-Criticising/Attacking & Self-Reassuring Scale
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(This has been removed from the electronic copy)

Appendix H

Positive and Negative Affect Schedule

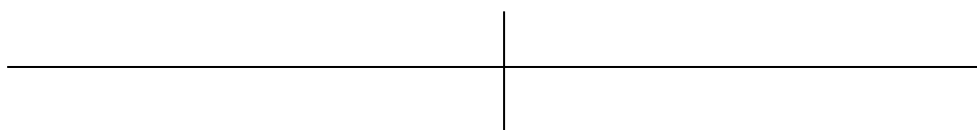
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Appendix I

Concentration measure

Please rate your concentration level over the last 15 minutes:

*I feel my
concentration
has been very
poor*

A horizontal line with a vertical tick mark in the center, representing a rating scale.

*I feel my
concentration
has been very
good*

Appendix J

Acceptance-oriented meditation transcript

So, spending a few moments now bringing awareness to your body and posture and making any adjustments that seem helpful. And knowing that during the practice our posture can change, and that it's fine to readjust so that our posture continues to embody a sense openness, awakesness and dignity, as best it can.

And closing your eyes now, if that feels comfortable, or alternatively having a soft gaze on the floor, a meter or so in front of you.

SHORT PAUSE

And during this practice, holding in mind that we're not trying to achieve any particular state, we're not even trying to relax during this practice, so seeing if it is possible to let go of the tendency that we all have to want things to be a certain way, and the tendency to judge how well we are doing.

Rather seeing if it possible to greet your moment by moment experience with a sense of openness and gentleness.

And now, bringing attention to the breath, perhaps to sensations in the belly or chest, as they expand with the in breath and contract with the outbreath... or perhaps to the passage of air in and out of the mouth or nose, noticing maybe the difference in temperature between the inbreath and outbreath. Or perhaps placing attention somewhere else, where the breath sensations are particularly accessible and vivid for you right now.

As best we can, not thinking about the breath, but rather being with the experience of the body moving as we breathe.

And if you find it a struggle to locate this experience then perhaps resting a hand on your chest or belly for a few moments now, and watching it move with the breath.

And, if you have not yet done so, now returning your hand to where it was before, and continuing to watch the breath.

PAUSE

And throughout this sitting meditation practice the breath will always be present as something that you can return to, if you find yourself overwhelmed by your experience during this practice. It's also fine to stop the practice at any point, if that seems the best thing to do.

PAUSE

And, as best we can, letting go of controlling of the breath, but rather allowing it to come and go as it pleases. But if this doesn't seem possible right now, then that's fine too.

PAUSE

And if the mind should wander away from the breath, knowing that that's absolutely normal and OK. Noticing where the mind has wandered to, acknowledging that, and gently bringing attention, back to the breath.

PAUSE

We may find judgments arise, judgments about the mind wandering. If this happens, as best we can, noticing judgements with kindness and allowing whatever arises in our experience to be just as it is. Remembering there is no right or wrong.

PAUSE

And inevitably the mind will wander from the breath. Perhaps to other sensations, perhaps to thoughts about the future or past, perhaps to some other aspect of experience.

This is completely normal and isn't a problem at all. When you become aware that the mind has wandered from the breath, notice what it has wandered to and invite in the possibility that it is ok that the mind has wandered, after all this is what our minds do. And then gently letting go of whatever it is that the mind has wandered to and shift attention back to the breath, as best we can. No need to push away experience, but rather just let it be, as you return to the breath.

PAUSE

So feeling the breath sensations as they flux and change, moment by moment, breath by breath, as best you can.

PAUSE

Knowing that, as it is the nature of all our minds to wander, if this is what we notice, this is fine. Noticing and experiencing the wandering mind is an important part of the practice, and is a valuable opportunity to practise gentleness and patience, as best you can.

PAUSE

So, noticing where your mind is right now and remembering that wherever our mind is right now, that's OK. And if the mind has wandered away from the breath gently coming back to the breath now if that feels OK to do so.

PAUSE

Seeing if it is possible to be as curious as you can be towards your experience right now, whatever that may be, exploring what is around right now for you, the change and flux, moment by moment.

PAUSE

So noticing your present moment experience, including the breath as it enters the body and as it leaves, and as best you can greeting the wandering mind with gentleness and patience.

PAUSE

So as best you can, watching the breath, moment by moment, breath by breath. And if the mind should wander away from the breath, knowing that that's absolutely normal and OK. Noticing where the mind has wandered to, acknowledging that, and gently bringing attention, back to the breath.

PAUSE

In a few moments time, I'll invite you to gently bring this period of sitting practice to a close. But for now, noticing what effect, if any, these words are having on your experience.

PAUSE

So, when you feel ready, opening your eyes gradually, if they are closed, bringing awareness back into the room, and slowly and gently making the transition from this period of sitting practice to whatever it is that you plan to do next.

Appendix K

Concentration-oriented meditation transcript

So, spending a few moments now bringing awareness to your body and posture and making any adjustments that seem helpful. And knowing that during the practice our posture can change, and that it's fine to readjust so that our posture supports our ability to sustain a concentrated state of mind.

And closing your eyes now, if that feels comfortable, or alternatively having a soft gaze on the floor, a meter or so in front of you.

SHORT PAUSE

And during this practice, holding in mind that we're aiming to achieve a concentrated state of mind, a mind that is singularly focused on the present-moment experience of breathing, so seeing if it is possible to let go of the tendency for the mind to wander, and instead focusing solely on the present-moment experience of breathing.

Rather seeing if it possible to hold a clear intention to remain present with the breath.

And now, bringing attention to the breath, perhaps to sensations in the belly or chest, as they expand with the in breath and contract with the outbreath... or perhaps to the passage of air in and out of the mouth or nose, noticing maybe the difference in temperature between the inbreath and outbreath. Or perhaps placing attention somewhere else, where the breath sensations are particularly accessible and vivid for you right now.

As best we can, not thinking about the breath, but rather being with the experience of the body moving as we breathe.

And if you find it a struggle to locate this experience then perhaps resting a hand on your chest or belly for a few moments now, and watching it move with the breath.

And, if you have not yet done so, now returning your hand to where it was before, and continuing to watch the breath.

PAUSE

And throughout this sitting meditation practice the breath will always be present as something that you should do your best to concentrate on. However, if you find yourself overwhelmed by your experience during this practice it's also fine to stop the practice at any point, if that seems the best thing to do.

PAUSE

And, as best we can, letting go of controlling of the breath, but rather allowing it to come and go as it pleases. But if this doesn't seem possible right now, then that's fine.

We may notice the mind wanders. If this is associated with a sense of restlessness or agitation we may find it helps to focus on sensations of the breath lower in body, such as bringing awareness to sensations in the belly as we breath. If mind wandering is associated with feeling sleepy we may find it helps to focus on sensations of breathing higher in the body, such as the movements of air through the mouth or nose. If mind wandering is associated with feeling bored it can help to bring curiosity to the moment-by-moment experience of the breath, noticing how each breath is unique.

PAUSE

So if the mind wanders, noticing what the mind wandering is associated with and using one of these techniques to allow us to achieve a greater focus on our present moment experience of the breath

So feeling the breath sensations as they flux and change, moment by moment, breath by breath, as best you can.

PAUSE

Knowing that it can be a challenge to sustain concentration and so if your mind wanders coming back to the breath, keeping in mind the intention here is to cultivate a concentrated state of mind.

PAUSE

So remembering if you're feeling restless you can lower the focus of attention to the belly, if you are feeling sleepy you can raise the focus of attention to the mouth or nose and if you're feeling bored you can bring curiosity to the moment by moment changing experience of the breath.

PAUSE

So, paying attention to the breath as best you can and noticing how the experience of breathing changes moment-by-moment, breath-by-breath. And if the mind has wandered away from the breath coming back to the breath now.

PAUSE

Seeing if it is possible to be as curious as you can be towards your experience of the breath right now, noticing how the breath fluxes and changes, moment by moment.

PAUSE

So noticing your present moment experience of the breath as it enters the body and as it leaves, and as best you can maintaining concentration on this experience.

PAUSE

In a few moments time, I'll invite you to gently bring this period of sitting practice to a close. But for now, noticing what effect, if any, these words are having on your experience of the breath.

PAUSE

So, when you feel ready, opening your eyes gradually, if they are closed, bringing awareness back into the room, and slowly and gently making the transition from this period of sitting practice to whatever it is that you plan to do next.

Appendix L

Second meditation introduction (pre-acceptance)

In the first practice we were attempting to achieve a concentrated state of mind by focusing on our present-moment experience of the breath. This next practice will now invite us to let go of that approach and instead be open to whatever arises in our experience during the practice, not aiming for any particular state. So just to be clear, in this practice whatever you experience is OK, if your mind wanders that's fine.

Appendix M

Second meditation introduction (pre-concentration)

In the first practice we were open to whatever arose in our experience during, not aiming for any particular state. In this next practice your aim will be to achieve a concentrated state of mind by focusing on the present-moment experience of the breath. So just to be clear, in this practice you are aiming to focus your mind on the breath and to minimise mind wandering.

Appendix N

Sitting posture instructions

So sitting on your chair and if it is comfortable to do so, sitting away from the back of the chair, so that your spine can be self-supporting. If possible, sitting with your back erect so that the crown of your head is pointing towards the ceiling or sky, allowing your head and neck to be balanced on your shoulders, and placing your hands on your knees or in your lap in a comfortable way. And inviting your shoulders to be relaxed and dropped. As best you can, allowing your posture to embody a sense of wakefulness and alertness, and a sense of stability and dignity.

In this way our posture during practice can help embody the attitude that we can bring to our experience in each moment as it unfolds. An attitude of openness, awakeness and dignity.

Appendix O

Study debrief handout – Site 1

Debrief

Thank you very much for giving your time to complete my research study. The data you have provided will be valuable in further contributing to the literature-base surrounding the positive effects of mindfulness meditation on mood, and in informing how mindfulness meditation might be altered to improve these positive effects.

The specific experimental aim cannot be given at this time in order to ensure that the findings from future groups are not affected by participants potentially being informed of the aim by previous completers. I would still ask that you kindly do not discuss the content of today's session with anybody outside of the group. If you would like to be sent a full explanation of the study's aims following completion of all groups, please send an email to XXX

Your wellbeing

It is hoped that the experience was a pleasant one for you. However, if you experienced difficult feelings or distress during the study session which you felt unable to discuss with the lead researcher, or you're continuing to experience this, the following are places which can provide help:

- Your GP
- XXX counselling service: XXXXX or e-mail [XXXX](#)

Your right to withdraw your data from the study

As outlined in the participant information sheet, you hold the right to request that your data be withdrawn from the study and destroyed. This will apply until the point at which all of the

collected data has been analysed. Should you wish to withdraw your data, please send an email to XXXX and your data shall be withdrawn from analysis, with all paper copies of scales destroyed.

Prize draw

Following the completion of all study groups, all participants who submitted their email address to the prize draw will be entered into a randomiser and four names will be drawn to win a prize of a £25 voucher each.

Requesting the study's results

If you would like a copy of the final research project, this can be emailed to you following study completion. Please request a copy by emailing me at XXXX

Further details or complaints

To discuss any aspect of the study or to raise a complaint, please contact one of the following:

Professor Paul Camic

XXX

**Research Director, Salomons Centre for
Applied Psychology**

**Study Supervisor, Salomons Centre for
Applied Psychology**

Paul.camic@canterbury.ac.uk

XXX

Appendix P

Study debrief handout – Site 2

Debrief

Thank you very much for giving your time to complete my research study. The data you have provided will be valuable in further contributing to the literature-base surrounding the positive effects of mindfulness meditation on mood, and in informing how mindfulness meditation might be altered to improve these positive effects.

The specific experimental aim cannot be given at this time in order to ensure that the findings from future groups are not affected by participants potentially being informed of the aim by previous completers. I would still ask that you kindly do not discuss the content of today's session with anybody outside of the group. If you would like to be sent a full explanation of the study's aims following completion of all groups, please send an email to XXXX

Your wellbeing

It is hoped that the experience was a pleasant one for you. However, if you experienced difficult feelings or distress during the study session which you felt unable to discuss with the lead researcher, or you're continuing to experience this, the following are places which can provide help:

- Your GP
- XXXX located in the XXX on campus (XXXX or [XXX](#))

Your right to withdraw your data from the study

As outlined in the participant information sheet, you hold the right to request that your data be withdrawn from the study and destroyed. This will apply until the point at which all of the collected data has been analysed. Should you wish to withdraw your data, please send an

email to XXX and your data shall be withdrawn from analysis, with all paper copies of scales destroyed.

Prize draw

Following the completion of all study groups, all participants who submitted their email address to the prize draw will be entered into a randomiser and four names will be drawn to win a prize of a £25 voucher each.

Requesting the study's results

If you would like a copy of the final research project, this can be emailed to you following study completion. Please request a copy by emailing me at XXX

Further details or complaints

To discuss any aspect of the study or to raise a complaint, please contact one of the following:

Professor Paul Camic

XXX

**Research Director, Salomons Centre for
Applied Psychology**

**Study Supervisor, Salomons Centre for
Applied Psychology**

Paul.camic@canterbury.ac.uk

XXX

Appendix Q

Mean DV values and standard deviations across time and condition organised by self-criticism

	Acceptance condition				Concentration condition			
	Pre mean (SD)		Post mean (SD)		Pre mean (SD)		Post mean (SD)	
	Low	High	Low	High	Low	High	Low	High
	self-criticism	self-criticism	self-criticism	self-criticism	self-criticism	self-criticism	self-criticism	self-criticism
Mindfulness	56.89 (13.3)	59.06 (16.77)	74.26 (15.68)	75.39 (9.46)	56.06 (15.84)	53.24 (12.36)	77.88 (11.08)	78.35 (12.34)
Concentration	77.16 (27.04)	85.12 (26.21)	89.21 (22.89)	93.94 (22.07)	71.19 (28.81)	65.71 (31.81)	95.94 (13.48)	97.47 (17.63)
Positive affect	25.68 (5.31)	26.56 (6.29)	26.79 (8.31)	25.94 (6.17)	26.56 (5.13)	24.41 (6.59)	24 (7.16)	24.76 (8.01)
Negative affect	12.37 (2.01)	16.67 (5.34)	12.42 (5.75)	12.83 (2.92)	12.31 (1.85)	17.53 (6.1)	12.19 (3.51)	13.41 (4)

Appendix R

Study summary for dissemination to participants



Adapting Brief Mindfulness to the Individual: Consideration of Trait Self-Criticism

Study Findings

Thank you very much for taking part in this study. As a reminder, you attended an hour-long session where you were asked to complete a series of measures on paper before following a 10-minute mindfulness meditation, followed by completing more measures, listening to an audiobook and following a second (slightly different) mindfulness meditation. We were particularly interested to see whether the type of meditation interacted with how you rated yourself on your tendency for self-criticism, to influence your mood. Thanks to people like you contributing their valuable time, we collected data from 70 people in total.

The main hypothesis of the study was that for people who showed a greater tendency to criticise themselves, the mindfulness meditation which emphasised accepting and not judging any of your experiences (such as thoughts or emotions), would be preferable and result in a greater improvement in mood when compared to the mindfulness meditation emphasising the cultivation of concentration on the sensations of the breath. For those people showing less of a tendency to criticise themselves, the opposite pattern was predicted (i.e. showing a greater improvement in mood following a concentration mindfulness meditation).

Unfortunately, through analysing the data collected to look for patterns, this main hypothesis was not confirmed, and people both low and highly rated on self-criticism showed a similar improvement in mood after each meditation. However, further exploration of these changes showed that people high on self-criticism experienced a greater improvement in their mood following each meditation than those lower on self-criticism. One interpretation of these findings is that those more prone to self-criticism are more likely to benefit from a brief mindfulness meditation, regardless of that meditation's primary focus, when compared to those less prone to self-criticism.

These results which you helped to produce have allowed us to learn more about how self-criticism influences brief mindfulness meditations and have also generated important new questions which

future research studies can answer. For example, whether the two forms of mindfulness meditation work in different ways to improve the mood of people higher on self-criticism. Ultimately, this type of research may be helpful in improving interventions for people with mental health difficulties such as depression, which involve mindfulness meditations.

Thank you again for your help with this project.